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VOLUME 1

RATS

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PREFACE

ANYONE who wishes to get rid of rats is strongly advised to obtain the government bulletins referred to in Chapter 1. This will be cheaper and better than trying to use the present volumes as a manual on rat control. There are other views, however, about dealing with rats. These views conflict with the official pamphlets (based on research by the Bureau of Animal Population) and are advocated in much more alluring terms. The private individual may take his choice of method; but where public funds and welfare are concerned the decision must be weighed more carefully. These volumes are intended to help those responsible for such decisions and to describe the viewpoint and techniques required to test existing systems and devise better ones.

Most of the chapters were first written in 1946–7, but it took until 1949 to complete the editing. The typescripts were then submitted for publication (as agreed) by the Agricultural Research Council; but because of a change in policy everything was sent back to us 18 months later. The problem of getting the work published at all was solved only through the generosity of the Delegates of the Clarendon Press and of a donor, who wishes to remain anonymous, who gave £2,000. This gift has already been acknowledged with gratitude by the University and I here add the very sincere thanks of my colleagues and myself. It now became necessary, however, to change the style of the work and limit it to about one-third of its original length, a job which few of the authors were in a position to do and which my own university work has delayed until now. I have had to adopt a fairly ruthless policy of cutting texts and depositing tables and must thank the authors for their kind acceptance of the changes. Those who have not actually worked on rat control may think that there is still too much detail; those who have will know that there is too little. The fuller data showed more clearly the complexity of the field work and the difficulty of reaching conclusions. However, workers in this country can consult our files, and others must in any case find out for themselves how far procedures may be applied elsewhere.

The arrangement of the chapters is planned as follows: strategy, materials, field methods, and general results of controlling rats by poison (Chs. 1–4); aspects of behaviour, especially towards baits (Chs. 5–9); results in particular environments (Chs. 10–14). Studies on methods of control other than by poison have had to be omitted (Ch. 1, § 9) and the literature since 1946 has not been fully covered.

I wish to thank Miss Enid Richardson and Miss Rosemary Rowse for their remarkable ability to read mutilated typescripts.

D. C.

BUREAU OF ANIMAL POPULATION
OXFORD
1 *September* 1952

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ABBREVIATIONS

A.R.C.	Agricultural Research Council
A.L.D.	Average Lethal Dose
Antu	α-naphthylthiourea
B.S.	British Standard
B.S.I.	British Standards Institute
B.S.T.	British Summer Time (G.M.T. + 1 hour)
D.B.S.T.	Double British Summer Time
L.A.	Local Authority
L.D.	Lethal Dose
M.A.F.	Ministry of Agriculture and Fisheries
M.O.F.	Ministry of Food
M.O.H.	Medical Officer of Health
M.O.S.	Ministry of Supply
P3	Protected Poison Point
P.L.H.A.	Port of London Health Authority
R.C.T.	Rat Control Test
S.A.L.A.M.	Société Anonyme Levant-Africaine Méditerranée
W.A.E.C.	War Agricultural Executive Committee

Jargon has been avoided as much as possible, but certain terms have been used to save space, e.g. 'arsenic' for arsenious oxide, 'rusk' for moist sausage-rusk. The meaning of terms such as these may be found by looking them up in the index and seeing where they are 'defined' or 'described'.

I

RESEARCH ON RODENT CONTROL BY THE BUREAU OF ANIMAL POPULATION SEPTEMBER 1939 TO JULY 1947

By CHARLES ELTON

1. INTRODUCTION

BY the spring of 1939, when it became finally clear that war with Germany was likely, scientific workers in universities were notified that they would be reserved from military service in order to carry on research. No direct guidance was offered, however, as to how biologists could best assist the country if war did happen. I therefore drew up and submitted to the Agricultural Research Council a plan for the Bureau of Animal Population to transfer the energies of its staff, on the outbreak of war, to applied biological research on methods of rodent control. Since its foundation as a University research institute in 1932, the Bureau had been mainly engaged upon fundamental research into the ecology of wild mammal and game-bird populations. Its staff was small, but had extensive and unusual experience of the methods and difficulties of ecological field work, while its library provided a specialized background for any work on mammalian ecology. Also, much of its research was indirectly connected with practical problems, particularly in the country. None of its staff had any previous experience of rat or house mouse control, though some knew a good deal about rabbits.

The plan put forward was to concentrate upon brown and black rats, house mice, rabbits, and voles. In the event, the first four were our main research, and work on voles was confined to the maintenance of certain valuable laboratory stocks. The aim was stated to be:

(*a*) Technical improvements in means of rodent destruction (traps, poison baits, &c.)
(*b*) Working out a general policy of control aimed at the greatest destruction with the least cost and labour. This would involve:
    (1) Contact with, and, if necessary, participation in organization (e.g. by the State, or by counties).
    (2) Some degree of intelligence system to keep track of the situation in different pest species.
(*c*) Adjustment of technical methods and advice about them to available supplies of equipment and material, which may become very different in war-time (1) because of stoppage of imports, (2) because of diversion of raw materials, labour, and factories.

It will be noticed that what is now usually called operational research was regarded as an essential feature. In other words, academic research needed to be supplemented and its points tested and driven home on a practical scale, and this in turn implied that the scientist should have a considerable influence in suggesting the lines on which the executive organization should be moulded. That it was possible to carry through this idea successfully was in great measure due to the A.R.C.'s willingness and ability to be an active interpreter to the other government departments of the scientific ideas, both upon technical methods and effective organization, submitted to it at various times by the Bureau's research team.

Early in August the A.R.C. accepted the plan in principle, and by the end of September 1939, i.e. about 3 weeks after the outbreak of war, the University also concurred. This arrangement, by which the Bureau undertook to act as a research and scientific advisory service on rodent control to the A.R.C. and through it to other government departments, was continued until the end of July 1947, although during the last of these 8 years a considerable amount of other research was started again. A useful account of some aspects of the war-time research of the Bureau and its application and development by the Government is given by Barnett (1947, 1948 *b*). Since August 1947 the Bureau has returned completely to long-term fundamental research on population ecology, in the University, where it forms a section of the Department of Zoological Field Studies.

2. STAFF

At the outbreak of the Second World War, the Bureau had a research staff of five people (including the Director). Others were added subsequently. Altogether seventeen research workers were employed at various times. In such closely knit team-work as this was, it is neither easy nor desirable to parcel out exactly the credit or responsibilities for different lines of discovery. Some of the special discoveries that seem to have reached flash-point in individual minds owe much to preliminary discussion and trial by the whole team or parts of it. Of this team, one (R. M. Ranson) died in September 1944 and his loss was severely felt by his colleagues. He can be counted as a war casualty, because he drove himself too hard in face of physical fraility. Four of us (D. H. Chitty, P. H. Leslie, H. N. Southern, and myself) have remained on the Bureau's research staff, which is much the same size as it was when the war began. Other colleagues were as follows: J. M. McC. Fisher (1940–4), Mrs. H. Flatt (1945), R. B. Freeman (1939–46), H. R. Hewer (1939–41), Miss E. M. O. Laurie (1943–8), A. D. Middleton (1939–45, seconded by Imperial Chemical Industries, Ltd.), J. S. Perry (1943–6), J. Rzóska (1940–6), Miss M. R. Shorten (1943–8), H. V. Thompson (1942–6), J. S. Watson (1941–9), and L. S. V. Venables (1939–45).

The main fields of each person's activity may be seen from the authors' names on the various chapters and from the references to published and unpublished work in the present chapter. Special mention must be made, however, of certain work which is not otherwise credited. Venables worked with us as a volunteer and contributed a great deal to work which has been written up by others. Miss U. Wykes (now Mrs. Venables) also gave her spare time to dissecting some 10,000 rats brought in from the field. Hewer, while on our staff, worked at Slough, at the Field Station of the Imperial College of Science and Technology by whom he was seconded. In April 1941 he accepted the post of Chief Rodent Officer in the Ministry of Food, which up to that time had virtually no rodent control organization. This was a most important and successful move, which gave to urban rat and mouse control policies a truly scientific basis for action. Hewer organized instruction of urban and military operators which fully drew upon the methods worked out at Oxford, and through his war-time work on what has since grown to rather a large administrative machine, now under the Ministry of Agriculture and Fisheries Pest Infestation Division, in effect helped to turn this country's patchy and *laissez-faire* system of urban rodent control into a highly geared organization. In particular should be mentioned the work he did at the Ministry in developing a small-scale demonstration of sewer rat control in Oxford, into a large organized system that operated over the whole of London and in many other cities and towns.

Subject-indexing of literature played an important part in our work. Freeman compiled and cross-indexed under subjects a very large bibliography of rodent control which, but for prohibitive cost and printing difficulties, would have formed a special volume in the present series. It is at present deposited in the Bureau. Acknowledgement is offered here of the very large amount of work done by him in preparing this bibliography, which ought if possible to be published.

Considerable work on rabbit control was done by Middleton and Southern (with some assistance from Freeman), but will not be further discussed in the present volumes.

As regards the office and library staff, mention should be made here of the sterling capabilities of Miss M. Gibbs, the Bureau's secretary and accounant since 1935, and of the excellent service given (under rather crowded and trying conditions of working) by Miss C. Fox (1940–5); Miss B. Olliffe (1943–7), Miss S. M. Weatherley (1945–51), and Miss E. Richardson (1945–8).

3. RAT AND MOUSE INFESTATIONS IN BRITAIN

Before proceeding to describe the war-time research itself, one must try to give some picture of the degree and variety of rodent infestation in this country in 1939 as we afterwards came to realize it. Rats and house mice occupy a great range of habitats, and most people are agreed that their extermination is desirable. The chief rat both in country and town is the brown or Norway rat (*Rattus norvegicus*), a large species living mainly in or near the ground, but also capable of climbing to higher levels with some agility. Its home is in China, and it did not spread here until the early or middle eighteenth century. This is the only important rat in farming country, where its powers of damage are, however, challenged by those of the house mouse. It infests farm buildings, banks, and refuse dumps all round the year; goes also into field crops in summer; and breeds in corn-ricks through the winter and spring. Less abundantly it occurs on the banks of waters, and on the shores of estuaries and the sea. But it is in urban surroundings that the brown rat finds the most extraordinary number of different habitats to live in. These include dwellings, back-yards, refuse dumps, shops, restaurants, hotels, offices, warehouses, granaries, railway premises, and factories; and they range from the warm conditions of brick-works, sewers, and coal-mines, to the low temperatures of cold stores. The harm done is correspondingly manifold in its nature, so that besides the destruction of all kinds of stored food, there is not inconsiderable damage to building structures, and to manufactured goods like fabrics, upholstery, leather, and soap.

And of course the brown rat carries diseases, of which the most important in this country are the bacteria of food poisoning, the organism of spirochaetal jaundice, and the nematode worm that causes trichinosis.

The black or ship rat (*R. rattus*), originally from India where it is still the chief roof rat and plague carrier, colonized Europe in the early Middle Ages. Formerly widespread, it was then destroyed on land, apparently by the spread of the brown rat. Though surviving as the chief species on ships, it has spread in recent years from its bases in the seaports to become the chief rat in central London, while it is also dominant in many portsides (Matheson, 1939) and is establishing itself, though still in small numbers, also in some inland towns and cities. This species can climb high and penetrate into roof-flats, grain silos, and upper floors of warehouses. Its damage is primarily to food, but it also follows the larger species in other sorts of depredations. It is particularly the species likely to introduce bubonic plague, because of its ship-borne habits. There is supposed to be competition between these two species of rat, and mixed populations are not very frequent; but their exact ecological relationship has still to be discovered.

The house mouse (*Mus musculus*) was originally an inhabitant of the steppes of Asia, but has lived closely with man in Europe for many centuries. It is found in almost as great a range of indoor urban habitats as the brown and black rats. What has been less widely known, except to farmers, is the remarkable abundance that mice reach in corn-ricks, where the damage done fully equals that from rats. In farm buildings they are less abundant though widespread. House mice attack many sorts of food, and also sometimes carry *Salmonella* bacteria that can kill man. Their penetrating power into buildings is much greater than that of rats, because of their small size and the difficulty of screening them out.

4. RAT AND MOUSE CONTROL IN BRITAIN BEFORE 1940

Before the Second World War, research upon rat and mouse control in this country hardly existed, and apart from some studies on taxonomy, distribution, and parasites, the biology of these species was incompletely known. Two examples of our ignorance then of the ecological problem that control presented, are the discovery only during the war of the universal distribution and abundance of house mice in corn-ricks, and of the continuous breeding of rats in ricks in winter when other habitats showed a considerable drop in productivity.

The organizations concerned with rodent control presented a somewhat less sketchy appearance, though of few of them could it be said that total extermination of rodents was either attempted, expected, or achieved. The charge of rat and mouse control was placed legally upon the occupier of premises, who was supposed to be kept up to the mark by the agents of 722 local authorities, and sixty-two port sanitary authorities. These were given certain powers, the former under the Rats and Mice (Destruction) Act of 1919—a legacy of our food conservation campaign in the First World War; the latter also under international sanitary conventions, impelled in a large degree by the fear of bubonic plague.

Outside the seaports the 1919 Act was primarily administered by the M.A.F. (or equivalent bodies in Scotland and Northern Ireland). The Ministry had a Technical Adviser, who paid some attention also to pests other than rats and mice, and its best-known public gesture was the organization of a 'Rat Week' in November. At the outbreak of war the adviser's job was vacant through his retirement. The port sanitary authorities were primarily interested in the control of rats on incoming vessels, and were answerable to the Ministry of Health, who also had a direct connexion with local authorities in that the latter's rat and mouse control activities were mostly done by sanitary inspectors under the local medical officers of health. In farm areas there was no government system of carrying out rat and mouse control, except where a few particularly active county councils took this job seriously, and perhaps offered some service to the farmer.

The actual work of rat and mouse killing was done by a wide variety of individuals, including the agents of local authorities or of large industrial concerns like the railways; the ship inspectors or rat officers of port health authorities; private servicing firms; free-lance rat-catchers; the public (including its private agents, such as gamekeepers); and salaried cats (whose weekly milk or meat consumption was an item on the account books of some large concerns). These activities depended in turn on a fairly considerable chemical industry. More than a hundred competing firms sold poisons, bacterial cultures, traps, and other devices, often to the accompaniment of confident and even lurid advertising. Some of these firms had an association, one of whose aims was to watch the legislation and administration of poison laws.

Three of the tenets of population ecologists concerned with modern applied biology are (1) that the success of pest control should not be judged by subjective impressions, (2) that the removal of a part of the pest population does not necessarily result in control, and (3) that spasmodic control at high densities of the pest is often wasteful and even misleading in its apparent effects. Failure to follow the first of these accounts for the perpetuation over many years of some control methods that do little or no good. The second principle means that partial (even quite heavy) mortality of the pest may produce only a slight effect, if it makes population density conditions more favourable for the survivors. Thirdly, the treatment only of outbreaks and of complaints leaves out a huge population of rodents continuously doing damage less conspicuously, and encourages a patchy treatment of the problem. In some instances decrease after high density is due to natural causes and not to control at all.

We were, at the outset, compelled to face a very unsatisfactory situation, in which the anti-rodent forces of the country were often organized only on paper; the efficiency of their work was judged almost solely by subjective impressions (often of interested parties, who however sincere, might be mistaken); the supplies of chemicals were largely unstandardized and the methods of using them partly secret; the undoubted existence of large and numerous infestations of rats, mice, and rabbits testified to the failure of these forces to achieve general control in spite of some local successes (of which ship rat fumigation, and the clearance of State Forest planting areas of rabbits were notable); and the lack of organized ecological and pharmacological knowledge was apparent. To these difficulties must be added our own initial ignorance of this field, except on the general ecological side. The first steps had therefore necessarily to be reconnaissance.

5. RECONNAISSANCE

In §§ 5–10 attention is concentrated on the research we did upon rats, although work upon house mice was also going forward in the early years of the war (see § 11). For the first 7 months we were restricted by our contract with the A.R.C. mainly to rural rat problems. But it was soon realized and accepted that the whole field of rural, urban, and port rat population control had to be investigated. Damage done by rats goes on in the standing corn and many other departments of the working farm; in corn-ricks and farm meal-stores; in rural mills and warehouses; in railway and dock warehouses; in bakeries and slaughter-houses; in provision shops, restaurants, and hotels; in dwelling houses, gardens, and allotments; in sewers and river banks; and in bone-yards and refuse dumps—to mention only a few of the habitats in which rats do appreciable harm to food, merchandise, and structures of various kinds. The Bureau has records of brown rats from such odd places as a coal-mine at 1,800 feet depth, the painting section of a large motor factory, warm brick kilns, a film studio and a chapel used as buffer depots for grain, and the Bodleian Library.

A preliminary inquiry by letter in November 1939 to the sixty administrative counties of England and Wales, and several personal surveys of farm groups in the Oxford region, made it plain that the rat infestation was widespread on farms and was particularly noticeable in corn-ricks. At the same time, pest control organization to deal with it, public or private, was thin and patchy to say the least. Private servicing companies (see below) operated very largely in towns because of the heavy overhead travel cost of going to single farms. It appeared that keepers on estates having partridge or pheasant preservation were in the habit of doing a good deal of rat killing in field banks at certain times of year by means of prebaiting and arsenic; though they seldom tackled farm buildings or corn-ricks. (To give the scale of this habitat, it may be mentioned that one Norfolk estate of 2–3,000 acres has about 50 miles of hedge-bank on it.) Our later work showed that heavy field-bank rat infestations were commonest in the eastern counties. The corn-rick infestations were widespread, and other types of farm damage very common. Small rural or outlying urban refuse dumps were also usually occupied by rats.

Corn-ricks seemed to stand out as one of the keys to rural rat infestation and control both because of high density of rats, huge numbers of ricks and the possibilities of killing rats in them, and also because they provided more or less discrete populations for ecological and reproductive studies living in a rather simple kind of environment. During the next 3 years a considerable study of rick populations was made, and they were indeed found to be the sorest spots on farm areas, because in them rats can carry over the winter with a high breeding potential and in food and security (see § 10).

The first practical result from this work was the crystallization of a casual country practice into a legal obligation, by the Rats Order (No. 2), 1940, issued in August 1940, which made it compulsory to fence round ricks at threshing time and kill the rats. In August 1941 the Rats Order, 1941, made rather clearer who was responsible for doing the job.

Meanwhile similar reconnaissance by Hewer was going ahead in towns. Sample surveys were made of infestations in 100 Oxford business premises, and in Reading. In June 1940 (not long before the blitz began) a very thorough inquiry was made, in conjunction with Fisher, to the local authorities in fifty London boroughs. Contact was also made with every active firm (except some of the chemical wholesale manufacturers) engaged in selling poisons, 'virus', or fumigants for killing rodents; and a sample survey done of the poisons sold most commonly in retail shops. Visits were paid to Hull, Bristol, and Liverpool to study how port rat control was carried on.

Certain facts began to stand out especially clearly. The vacuum caused by the absence of any comprehensive state-guided organization had been partly filled by half a dozen large servicing companies. These firms chiefly served industry, both manufacturing and retail, and their business was a flourishing one. They mostly relied upon red squill and 'viruses' (really cultures of *Salmonella* bacteria), and they worked on a contract basis. The community generally, but more especially the general public, was also buying enormous amounts of poison, sold chiefly through pharmaceutical chemists, and this was preponderantly mixtures containing phosphorus. The local authorities did comparatively little to help industry or the farmer, being occupied mainly with the complaints of householders and shopkeepers, and above all with the sewer rat. Their methods were various, but they were apt to look askance at the use of *Salmonella* cultures.

It was natural to suppose that all this activity, representing probably well over half a million pounds worth of work a year, would be found to rest on some ascertained skills, tests of the methods used, and perhaps a biological philosophy. The greatest courtesy and help was extended to the Bureau's staff by the servicing companies and other firms, and by the representatives of local and port health authorities. But, as I shall show in §§ 6 and 7, in the end we had to check over every statement or assumption ourselves by methods as objective as we could make them, and this resulted in considerable changes in the whole edifice of rat control methods and organization, except on ships in harbour, where fumigation was well understood. Empirical views sincerely held are unfortunately no substitute for cold proof, and as far as the rodent control industry was concerned, we had (with one or two notable exceptions) to come to the conclusion, not only that their ways were not our ways, but that our ways were only likely to be operated by a state-guided organization, which would have all the advantages of a fresh start, a scientific basis, and uniformity of techniques, though also naturally the difficulties inherent in central state planning and devolution to local authorities.

It is not my object to trace in any detail the development of the national campaign organization itself in the war, but rather the research that underlay it. But it will clear the ground to say here that eventually the rural side became the task of pests officers and staff working for the War Agricultural Executive Committees under the advice of the M.A.F.'s increased staff of technical advisers; the urban and some small rural authorities were organized to action and their staffs trained by technical members of the M.O.F., with its new Infestation Division; while many port health authorities were introduced to some new methods for control of land rats, by the practical example of the Port of London Health Authority. Broadly speaking, the rural organization in England and Wales had got properly on its feet by 1943, the urban by 1944, and new developments in the Port of London already by 1942—though naturally a great deal was tackled in some areas before these dates. The A.R.C. linked the Bureau of Animal Population to these organizations, including government activities in Scotland, which had its special arrangements differing in many ways from those south of the Border.

All this reconnaissance led to some very simple and clear decisions on the research side. We would make a special study of rats in corn-ricks and rats in sewers as well as continuing investigation in other habitats. There was a great variety of ways in which rats could be attacked, but for a very big national campaign, and for concentration of research, we must look to a few methods that could be made universal, simple, and standard, and that would be workable within the bounds of war-time supplies of labour and materials. It very soon appeared that, except with direct killing of rats at threshing, poisoning methods were far more important than the others (like trapping, gassing, dogging, cats, bounties, and rat-proofing), which came to be regarded as subsidiary or accessory or—as with bounties—useless. Like Daniel, when he proposed to destroy the Dragon Bel with a highly effective poison bait, we cried: 'But give me leave O king, and I shall slay this dragon without sword or staff.' By 1940 the entire centre of our rat control research was poisons and baits and baiting, and the behaviour of rats in this connexion.

6. POISON SUPPLIES

There are three main classes of poisons used in rat baits: the so-called 'viruses' (i.e. *Salmonella* bacteria), red squill, and inorganic poisons like barium carbonate and arsenic. The first two were the main reliance of servicing companies, because of their reputed safety to man and most animals. The *Salmonella* question had already been looked into by the Bureau, in connexion with the possible control of voles. Knowledge was brought up to date by a wide search of the literature about uses and dangers of these cultures; and by bacteriological 'typing' of the strains in use against rats and mice in Britain. Leslie (1942) showed all six strains to be identical serologically, so far as laboratory tests could go, with the bacterium *S. enteritidis* (Gaertner) that causes food-poisoning in man. Some of the strains were reported to have caused illness and deaths in man and domestic animals when used abroad, though so far no undoubted outbreak had been traced to them in this country.

We therefore decided to recommend that 'viruses' should not be used in the national campaign, a viewpoint already adopted by most medical officers of health. But this was bound to increase the pressure upon supplies of red squill, which is a powder or extract of a Mediterranean bulb that has the useful property of being very unpalatable to mammals and also almost harmless to man, cats, and poultry, and some other species. Its harmlessness depends partly on emetic qualities and partly on non-poisonousness. Rats eat baits containing it, cannot vomit, and will die of it. Red squill presented, however, two difficulties. It had to be imported from the Mediterranean, which was a costly business, and during part of the war impossible; and its toxic properties were very variable and required bioassay, since no direct chemical measurement could be used. This meant keeping a stock of standard white rats for laboratory toxicity tests, and as the squill was bought in bulk, some of the poor quality material that arrived from Algeria had to be then rendered fit for use by making extracts to concentrate the toxic powers. The long series of research operations that were done with red squill is fully described in Chapter 2. In the end, we knew where to get the best supplies, how to test and if necessary process them to a standard product, and something of the degree of danger and efficiency of this poison. On the whole, red squill proved to be less useful than is generally thought; indeed one of the big changes in poison use against rats has been the diversion from 'viruses' and red squill to inorganic chemicals.

It very soon became clear that we were involved in a triangle of considerations that had to be satisfied: *safety*, *supplies*, and *efficiency*. In Britain there is a rather careful supervision by the Government of poisons and their use, and public safety in this respect has a high value that rodent controllers need to obey. The supplies were affected by import restrictions, by the need to use *standard* poisons (implying continuous and large supplies of one sort) and by rival industrial demands. Efficiency depends on the qualities of the poison, and on how it is administered in the field.

Barium carbonate, an old stand-by of government departments at home and abroad, occurs in the raw state in some large British witherite mines, but we soon found it was highly dangerous to cats, and in any case probably rather inefficient through its relatively low toxicity to rats. It was not used on a large scale except in sewers.

Arsenious oxide, an even older standby, of the gamekeeper, could be got from home or abroad, but in many different grades, involving some bioassay. It was efficient, and used in the right baiting places far safer than its medicolegal reputation would imply. It became one of the chief poisons for baiting.

Zinc phosphide, more toxic than the last, was efficient and came into wide use after 1943 as the alternative to arsenic. It carried some very real dangers that needed close watching. The supplies were British, but its phosphorus was needed also for war devices.

Phosphorus itself was ruled out for our general purpose because of the danger (both from poison and fire risks) of making up baits anywhere except in a proper factory.

Thallium sulphate was unobtainable because import ceased during the war.

(*Strychnine* was and is forbidden by law for rodent control.)

Some other inorganic materials were looked at but turned down for one reason or another. Our task eventually came to be ensuring standard supplies of arsenious oxide and zinc phosphide and using them in ways that would be efficient and safe.

In the United States, two organic compounds were developed as new rodenticides during the latter part of the war, and sample supplies were received by us for testing in 1943 onwards. α-naphthylthiourea (Antu) was brought forward by Dr. Richter and his group at the Johns Hopkins University, Baltimore. Tests by us confirmed its efficiency, though not by the direct poisoning method. Its extremely high toxicity, and the absence of adequate proof at that time of human immunity to it prevented it being used here during the war. But it has had an expanding use since, though it is dangerous to some domestic carnivores.

The U.S. Fish and Wildlife Service brought forward sodium fluoroacetate ('1080'), after a very big research testing of hundreds of possible poisons. This is even more toxic than Antu, but unfortunately highly dangerous also to the host of the rat—man, and to dogs, &c. We refrained from studying this poison and in the United States it is still officially regarded as one to be used only under rigid specialist control. It has killed a few people already.

The sorting out of poisons, and the search for the best compromise between safety, availability, and efficiency, occupied a large section of the Bureau's energies between 1939 and 1943.

After a rather chaotic period, very good arrangements were made from 1943 onwards with the Ministry of Supply's Raw Materials Department by which each bulk supply of rodent poisons was discussed with the Bureau, who usually did sample tests on white rats and also ran a short field series of tests on wild rats. Help was received from the Government Chemist's Laboratory in the analysis of chemicals concerned.

White rat breeding stocks held at Oxford for bioassay were only sufficient for a limited number of tests; and equally, the field tests could not be too large or prolonged. The first difficulty was partly met by doing very careful statistical analysis of the small numbers of rats used in a test, and partly by getting some help from the laboratories of Imperial Chemical (Pharmaceuticals) Ltd. in Manchester; the second required us to prosecute research on the validity of field testing methods. We were always working against time, and in spite of this, trying also to shore up our empirical determinations by an understructure of more fundamental research upon the ecology and behaviour of rats.

7. FIELD METHODS OF POISON BAITING

It did not dawn on us for a little time that the then existing methods of preparing and laying baits might need drastic revision. In consequence, much of the first year's research on this aspect was confined to the general survey of poisons already described and to developing new devices to save labour and increase safety. We would at this period have been surprised if we could have foreseen that the national campaign staff would 2 years later be killing rats by methods that involved a great deal *more* labour initially and using poisons previously regarded as highly dangerous! The last change has been partly explained in the previous section; the first one needs expanding, as it concerns what was undoubtedly a major research discovery and the core of the whole rat control policy in this country, crystallized during the war into a standardized and generally adopted system of attempted extermination.

During the first year or more after the outbreak of war, field tests with poison baits were confined to the method known as 'direct poisoning', i.e. the laying of a food containing poison without previous baiting with plain food. At the same time various tunnels, pipes and boxes were tried out as specific protective devices, the most successful being 'P3', the abbreviation at first for 'Permanent Poison Point' (Ch. 3). Assuming, as we did then, that direct poisoning was a perfectly efficient method, it seemed logical that, after technical study, a semi-permanent bait could be produced, and if this was protected from all but rats and mice, control could be automatic and save the labour of repeated bait laying.

Furthermore, as rejection of bacterial cultures and any scarcity of red squill would drive people to use inorganic poisons, it seemed that devices would be needed to reduce the dangers attaching to them. At the same time, attempts were being made to use various trace indices for measuring the relative sizes of rat populations before and after field experiments. In spite of occasional indications of local success these field trials with direct poisoning, both in and outside P3s, turned out failures. Gradually the conclusion was reached that rats might be getting conditioned against the baits by taking sublethal doses of poison and afterwards associating some ingredient of the bait with their unpleasant experience. This period was one of considerable frustration and disappointment, though it contained in it the seeds of the next stage. This stage is so fully described and analysed in Chapter 4 that it will be unnecessary to go into it here except in outline.

The discovery that rats could be fairly easily induced to feed increasingly upon plain baits, and could thereafter be poisoned with consistently higher success than by simply using direct poisoning, led to a surge forward in field research. It had already been appreciated that the success of control methods must be measured in the field, and could not be predicted from laboratory work. Prebaiting also supplied the missing key by which the difficulty of measuring relative population sizes was to a large extent solved, though it necessitated the closest and most constant scrutiny if misinterpretation of results were to be avoided.

Out of the activities described in this and the previous section, there had emerged by the end of 1941 a definite form of attack on the rat, supported by a mass of experiment that was partly done at an empirical level, but also beginning to probe into deeper causes. It was now possible to kill about 90% of a brown rat population; but much needed to be done before one could be certain that this method would work well in all the different kinds of habitats. It was important, too, that methods should be standardized and simplified as far as efficiency would allow.

As time went on, numerous subsidiary questions arose and were in turn answered. The food consumption rate of an average brown rat population was measured. The palatability of different cereals and other bait foods was assessed: supplies of these were of course very tight, and some pressure was needed to convince government agencies that it was going to be a good investment to use flour and sugar, biscuit meal, bread mash, and even soaked wheat, to ensure really high kills of rats. As experimental results piled up, analysis could be made of some of the different factors influencing success or the design of field tests. Some habitats, such as food depots containing flour, offered special difficulties in baiting, but most of these were met by offering grain soaked in water. The traditional gamekeeper's method of baiting in holes was exhaustively tested (1942 onwards) and standardized for rural work. It was also shown (1942–3) that rick populations could be successfully poisoned, but unfortunately only with a danger of leaving poison residues in the rick. An ingenious labour-saving device adapted bait-laying to the town sewers (1942–3), where rat populations not only form a reservoir capable of invading buildings above, but of fatally infecting workmen with spirochaetal jaundice (leptospirosis). Finally, the relative efficiencies of direct poison baiting and of prebaiting and poisoning were very carefully compared, and the unreliability of the former objectively proved, at any rate in the given conditions and with the given poison bait substances.

It was quite a big proposition in itself to find colonies of rats in sufficient numbers that would answer the criteria—such as isolation, accessibility, and absence of previous poisoning—required in a scientific test. Each locality where such a colony was visited or worked upon was given a code number for record, and during the war about 550 such locality numbers were entered on the maps of Oxfordshire and Berkshire. Altogether about 1,000 Rodent Control Tests (R.C.T.) were done in the field (or towns) and 100 in outdoor cages, and all the details were collated by means of punch-sorting cards. A hundred and fifty of these tests were done in London, mostly inside the Port Area.

This work led us into some queer places, which were not, however, without a certain sombre attraction, or even romance, in so far as they were part of the immense variety of human habitats that exist in this country, and which at the same time provides conditions for rats to live in. Some of the fundamental observations on rat behaviour were made on refuse dumps. Others were recorded, by day and at night, in part of a small decrepit pig farm on the outskirts of Oxford, which could not except by extreme courtesy be dignified by the name of 'laboratory' (Plate 4(a)). Here feeding behaviour, feeding rhythms, conditioning, and marking individuals, were studied on a population of wild rats that was often upwards of a hundred strong. The direct observation of these rats was not only fascinating for research, but also for people that received instruction from the Bureau. Some photographs of these rats feeding were taken here in complete darkness by means of infra-red radiation (Southern, Watson, and Chitty, 1946).

It has been said above that a stage was reached at which a reasonably careful operator could kill about 90% of the rats in a colony. The Bureau adopted an arbitrary level of 85% as the minimum acceptable mortality. Figures were usually higher, and not seldom reached 100%. But there remained the knotty task of eliminating the residual survivors—the 1–15% —, and of finding out just why a small number so often still escaped control. Much research and thought went into this problem, and the theoretical aspects are still not entirely cleared up. Practically, it was found that a complete change of bait—food base and poison—applied in a second treatment would usually get the final numbers down to a very low point or to extinction. A considerable laboratory investigation was done on the question of bait shyness and upon the memory of rats for unpleasant associations resulting from sublethal dose of poisons. This is described very fully in an unpublished work by Rzóska (summarized in Ch. 8, Appendix 1) and its implications to field tests are discussed in Chapters 4 and 10. It is sufficient to say here that this part of the work illustrates as well as any the intimate connexion between the pharmacological, behaviour, and population aspects of the problem. Perhaps the most interesting discovery was that rats did not always associate their previous poisoning experience with the poison but with the food base in which it was mixed.

We therefore arrived at the idea of prebaiting and poisoning, repeated when necessary with a completely different bait.

8. OPERATIONAL RESEARCH

Most of the field tests already mentioned had been done on single rat colonies or small groups of colonies. The number of rats involved was usually less than 100 and seldom over 200. Such experiments were good enough to use for framing the immediate answers to various practical questions that were needed. The methods evolved were such as practical operators could use in similar conditions. And a series of official recommendations was sent out by the M.A.F. to the pests officers of War Agricultural Executive Committees, and by the M.O.F. to its rodent officers in charge of the clearance of rats from food stores. But the Bureau had its eyes on complete extermination over wide areas, i.e. block control. It seemed desirable to carry out some operational research on this larger scale. In army parlance 'operational research' means the complete research analysis of all ways of planning and carrying out army operations. The research worker studies a huge and complex existing system and tries to improve it. In our case we were really setting up the basis of a new system of rodent control, almost from scratch, and so 'operational research' meant not only testing out a method on a large scale, as it would be done in practice, but also recording the process in detail and sometimes running experimental comparisons. The efficiency of such operational research tends to be a bit higher than that of a routine organization in so far as the supervision may be more skilled and the working staff a little more picked. It proves that a practical target can be achieved, not that it necessarily always will be by others.

There were four main series of special operational field tests—on farm land (Ch. 10, § 5), a town market (Ch. 4, § 16), town sewers (Ch. 12), and a port area (Ch. 13 and below); and a fifth investigation to try and measure the total results of the campaign in rural areas (Ch. 11).

One of the largest control operations supervised and recorded by the Bureau was in the London docks. In the summer of 1941 Dr. M. T. Morgan, the Port Medical Officer of Health of the P.L.H.A., suggested that the Bureau should send two research workers to record rat control operations and assist in applying new methods developed at Oxford. Some of the results have been published by Morgan, Fisher, and Watson (1942, 1943). Only a brief note will be given here of what was a very complex and very thoroughly recorded operation, lasting (as far as research was concerned) until September 1944. The area studied comprised the five dock groups in the Port; and records were also kept of the rats killed by ship fumigation, though no actual research was done on ship rat control. This area, it should be explained, does not cover the numerous wharves and docks, &c., along the river that lie outside the Port, but only those owned by the Authority. These, however, are considerable, and in 1941 included 542 different 'premises', i.e. transit sheds, warehouses, cold stores, grain silos, &c. Apart from a good many special experiments the actual control was done by the P.L.H.A.'s own staff of rodent officers (of whom there were, e.g. in 1941, nine). These men are highly trained inspectors who are able from long experience to make good estimates of the size of rat colonies.

The main operations fell into three stages. At first, it was found that nearly all control was done by trapping, as it had been for many years. There had been on the whole a gradual drop in numbers caught over the years, but in 1941–2 some 25% of premises still had rats in; and the total numbers of *Rattus norvegicus* and *R. rattus* taken on land in the Area in 1939–42 were 4,164, 4,157, 3,934, 3,944.

The second stage (Ch. 13) was to apply this poisoning system to a very large electric equipment factory at Woolwich (actually outside the Port Area), in which several thousand workers, mostly women, were exposed to the fear and risk of bites by an introduced tropical rat mite *Liponyssus bacoti*, carried by a widely spread population of black rats. This area, covering forty acres with eight large factory blocks as well as outbuildings, had no effective control system for rodents. Between December 1942 and March 1943 an almost complete clearance was effected, and few or no further complaints about the mites occurred. Follow-up maintenance treatment was so effective that in January 1944 it was estimated that fewer than fifty rats were left in the whole factory area.

During 1942–3 Dr. Morgan decided to introduce prebaiting and poisoning also into the whole Port Area, partly on a contract basis in the case of tenants of the Port Authority, and this third stage resulted in a considerable diminution of rats with the result that by June 1943 it was certain that no premises still harboured any but very light residual populations.

This London work was important in the broader sense that the black rat is the main plague-carrying rat in the world at large, especially in warmer countries. The convincing proof that it could be controlled by some of the standard methods employed against the brown rat, prepared the way for the research in the Middle East described in Ch. 14. This advance in the Bureau's work was substantially made possible by the great interest and vigorous energy displayed by Dr. Morgan and his consistent friendly help during these 4 years. The whole story recorded in the Port is not contained in the present volumes, though parts of it are described there and in the papers mentioned above. But all the details are deposited in the Bureau for reference.

9. SUBSIDIARY CONTROL METHODS

Although poison baiting had seemed, and eventually proved, to be the most decisive way of tackling rat infestations on a country-wide scale, there were many other ways of killing rats for which someone or other would be making confident claims, or which might be good in the hands of particularly skilled operators, or which might indeed be the only way of clearing the residue of some particularly refractory population of rats. The Bureau could not afford to investigate every one of these methods; but it was possible to judge of the qualities of some of the more important ones by exploratory research, and in a few instances to follow this with more elaborate work. The results of these and of many minor inquiries into other subsidiary methods have been omitted from the present volumes. It is hoped to publish some of them as separate papers.

Cyanide gassing is of course the standard and very efficient way of killing rats on ships when they come into harbour. The Bureau was never much concerned with this method as it could seldom be employed on land, except that if any building were fumigated by this method for insect pest control, rodents would automatically be killed.

Cyanide dusting, by pump or spoon, is not infrequently employed against hedge-bank rats, and had been recommended commercially for killing them in corn-ricks. The first claim was substantiated by Venables in field tests, in which results were assessed by counting the number of holes reopened after successive blockings and gassings. But the cost seemed rather high and the labour great if extermination was wanted. Cyanide dusting in corn-ricks proved quite unreliable, and also there are probably real dangers from cyanide residues.

Trapping was not only commonly used for eliminating rats, but also held out prospects of getting independent census figures in poison baiting tests and for ecological research generally. (It has, for instance, been notably successful in field research on voles and wild mice.) Observations on a small farm for over a year by Venables showed that invading rats could be exterminated regularly by gin-trapping—but it needed a high standard of attention and was unlikely to be practicable on many farms.

A considerable operational analysis of the history of trapping in the Port of London led to the conclusion that this was a less effective means of control than poison baiting, with which it was replaced in 1942 (see § 8). Any given number of traps and trappers seems to establish an equilibrium in which the numbers killed are balanced by immigrants, births, and possibly trap-shy behaviour (Morgan *et al.*, 1942, 1943, and Watson, unpublished).

Live-trapping was investigated by several members of the Bureau at different times, but we never managed to get really satisfactory and reliable results either with existing types of trap or with new versions of the funnel wire-trap that were tested very extensively by Thompson. It was concluded that, although trapping was undoubtedly useful and even essential in certain circumstances for final control of rat populations, it did not give a reliable index in field research owing to the variable behaviour of different rat colonies and individual rats. But this research gave strong supporting evidence to the parallel discoveries on rats' reactions to changes in their surroundings.

Bounties have from time to time been offered by some of the local authorities, for heads, tails, or bodies of rats—indeed the amount of money paid out by them over the years would have supported a very flourishing research institute. In other countries this practice of offering rewards to the public has also been practised a good deal and very often abused. Analysis by Freeman pointed to the conclusion, already strongly suspected, that the bounty system stands condemned both by its assumptions and its practical results.

Cats are frequently employed on farms and in shops and warehouses to keep down rats and mice, but no scientific study had been made of their real efficiency. A group of farms on the A.R.C.'s Compton estate provided some interesting information from which I concluded that cats could certainly keep a farm clear of reinvading rats after a bad infestation had been destroyed by other means, though at a high cost in food (e.g. milk) for the cats.

10. THE BIOLOGY OF RAT POPULATIONS

In the course of all this research an enormous amount of information was accumulated about the biology of rats. This has been indexed fully in the Bureau's punch-card system, but some aspects of it have also been analysed for publication. The material about feeding rates, feeding behaviour, general attitude to the environment and to changes in it, fixing of habit rhythms, and reactions to poisons, is so inextricably connected with the theory and practice of rat control that it has been set forth rather fully in these volumes.

There is also a considerable body of facts about the general population dynamics of rat populations: their breeding rates (and components thereof such as sex ratio, pregnancy, family size and frequency), population structure, density of numbers, movements, all subject to seasonal changes. The largest body of records is for reproductive rates and population structure. Over 10,000 individuals of *Rattus norvegicus* from the Oxford region were examined and recorded on punch-sorting cards (a copy of which is reproduced in the paper by Laurie, 1946). The majority of the rats were collected from corn-ricks, which offered especially attractive opportunities for the analysis of comparatively isolated whole populations. It is very seldom that the ecologist can get complete populations of small mammals for such a study, and this material was extensively used by Leslie not only for direct research on rat problems, but as a starting-point for a series of important theoretical papers upon the methods of treating such data with a much wider application.

A preliminary account (Venables and Leslie, 1942) dealt mainly with population sizes and established an important difference between those in corn-ricks, which breed throughout the winter at a high rate, and those outside which practically cease to breed then. A more complete monographic treatment of the records for three seasons, involving analysis of records for about 7,000 *R. norvegicus* has recently been completed (Leslie, Venables, and Venables, 1952). It is shown, for instance, that the rate of increase in corn-ricks, at least in some years, probably reached 8–10% per week. In two studies (1945, 1948) of the mathematical methods for handling mortality, fertility, and rates of increase, Leslie had developed some new methods, and incidentally used the brown rat as a 'model' for calculating inherent rates of natural increase.

An interesting application of the Bureau's experience of poisons was the use of the biological assay method to calculate the median body weight at which a female *R. norvegicus* or *R. rattus* attained sexual maturity (Leslie, Perry, and Watson, 1945).

Fisher and Watson in the Port of London collected card records of rats from ships and from premises in the dock area, using the laboratory facilities kindly provided by Dr. Arthur Davies, Pathologist at the Seamen's Hospital, Greenwich. Over 4,000 *R. rattus*, together with over 1,300 of the same species later collected in Cyprus, have been analysed by Watson (1950) to show the variations in breeding and other biological components.

Although the population structure and breeding rates are now rather well known from these studies, and from the independent work of Perry (1945), we still need information about the natural mortality rates of rat populations and about their movements, neither of which subjects could be studied extensively during the war.

11. RESEARCH ON THE HOUSE MOUSE (*MUS MUSCULUS*)

At the beginning of the war it was not supposed that house mice amounted to a serious pest in this country. The corn-rick surveys soon showed that it was, on the contrary, one of the most abundant small mammals on British farm-land, and that it destroyed much grain in the ricks. A survey of grocers' stores in Oxford in 1940–1 by Elton brought to light further considerable infestations there (in one Oxford grocer's store the density of mice was about one per square yard), as did inspections of food warehouses by M.O.F. officers. At this stage, all that could be done was to recommend efficient methods of using break-back traps in urban infestations, and these were eventually adopted by the government departments, and also by some of the trap manufacturers. Further ecological study was then done on a 'pure culture' of house mice inhabiting an Oxford flour depot. In 1942 Southern, having completed a chain of investigations on the rabbit, transferred his attention to the house mouse problem. He was later assisted by Miss Laurie and Rzóska. This team carried out very exhaustive research lasting until 1946 on mouse populations of ricks, of houses and shops, and of cereal and flour depots, paying attention on the one hand to biology (numbers, reproduction, movements, behaviour towards food, and to new objects in the environment) and, on the other hand, to trapping, poisoning, and gassing control.

The main practical upshot of this work was the discovery that the house mouse differs from the brown rat in having a very small range of movement, in not being at all easily diverted and 'anchored' to baits by prebaiting procedures and in some of its reactions to poisons. Although it is much harder to control by poison baiting than rats, nevertheless such control is often possible, provided a sufficient density and distribution of baits is secured. But in corn-ricks such intensive poisoning is too dangerous. This impasse was overcome by an ingenious use of carbon dioxide gassing, which is non-poisonous to grain but can secure complete kills of mice. However, so far, this last method has been too expensive and laborious to adopt. But the nature of the problem is at any rate understood, and in urban habitats considerable control can now be attained by trapping or poison baiting. And it has again been clearly demonstrated that each pest species has to be studied on its own merits, with account taken of different ecology, behaviour patterns, and pharmacological reactions.

12. THE PROCESSING AND DISSEMINATION OF RESEARCH RESULTS

The usual way of arriving at a balance of probability about the truth of a piece of research is through its publication and subsequent criticism and independent appraisal by other workers. For an ecological idea or method, 10 years would be a short time in which to go through this process properly. During the war there was not time for this, and furthermore it was undesirable to tell the enemy how possibly to improve his own pest control.

Throughout the war, the Bureau therefore supplied Reports to the A.R.C. on each research project as it matured to a point where general practical application seemed justifiable (in addition, of course, to general progress reports submitted at intervals). There were over 100 of these B.A.P. Reports, which contained the main part of the material that has been redigested for the present volumes, and formed the evidence on which each step of the national campaign against rodents was built up.

In the later part of the war the Government set up arrangements for circulating the B.A.P. Reports to the Colonial Office, the Dominions, the United States, U.S.S.R., and China. From time to time some echoes of this dissemination of information reached us, and it is known, for instance, that prebaiting and poisoning has proved successful in the Union of South Africa, and that zinc phosphide was tried out and recommended in U.S.S.R. (Kalabukhov, 1946).

A manual on 'The control and extermination of wild rabbits' was prepared by Middleton and issued by the Bureau in 1940. By 1943 it was felt that the methods of rat control were crystallizing into a definite shape. In April of that year the Royal Air Force asked the A.R.C. for a manual of rat control. It was decided to write a general instruction pamphlet for government agencies. A draft was written by me in the autumn of 1943, and it is a commentary on the difficulty of getting agreement on a technical document even as simple as this and producing it in war-time, that it took exactly a year to discuss it, modify it, and get it into circulation. The pamphlet was eventually issued by the M.O.F. (*Extermination of rats and mice*, August 1944), and was designed for use by other civil departments and by the three Services. In May 1945 another, *Rat control on farms*, was prepared by the Bureau and issued by the M.A.F.

Sewer rat control was not covered by the first pamphlet, since the M.O.F. dealt with this by separate circular; and the Ministry have since issued a revised Bulletin (1946) incorporating both the earlier pamphlet and the sewer circular together with certain later improvements in both. This bulletin should be consulted by practical users. The Bureau also assisted the M.A.F. in the revision of their pre-war general bulletin on rat control, which may also be consulted (1946).

In 1943–5 the M.A.F.'s technical advisers on rodent control, nearly all the pests officers of War Agricultural Committees, and all the instructors of the M.O.F.'s Rodent Infestation Division, attended 2-day courses at Oxford. These personal contacts did much to create goodwill and an appreciation of the nature of research operations; and they were useful to the Bureau in providing first-hand information, and sometimes very valuable facilities for field tests in some of the counties.

At the beginning of the war the necessity of setting up a good system for collecting and classifying information was realized. The first step was to draft a classification chart with about 500 subject units covering the biology, ecology, and control of rodents. To this was added a list of poisons and fumigants likely to be wanted. The next step was to start raking the world's scientific and technical literature, especially that on rats and on poisons. The results were classified and sometimes abstracted, and arranged in coded reference books. Later on a more convenient system was devised which employed punch-sorting cards. As far as possible, the original publications, or copies, were also added to the Bureau's library. A great deal of unpublished information was also included, and a simple system of code-numbering all experimental and field notes and other 'research archives' enabled these also to be screened and classified with the rest. This treatment of the literature on rodent control is still included as part of the Bureau's library activities, in the course of which incoming literature on the British and European (and a few American) mammals and game-birds is recorded on subject punch-cards.

A bibliography on rodent control was also compiled during the war by Professor Tracy I. Storer of the University of California, working for the United States Government, and it is satisfactory to recall that the Bureau exchanged microfilm copies of its own bibliography with similar ones from Professor Storer, whose friendly co-operation we are glad to acknowledge. The large unpublished bibliography assembled by Freeman has already been mentioned (§ 2).

From the necessarily simplified and rather stream-lined account of 8 years' work given in this introduction, it might seem that each research worker had a clear-cut task assigned to him by an all-wise government direction, and that progress was made just by division of labour and by individuals plugging along with the projects assigned to them. The organization and atmosphere were completely different from this, and I wish to set on record that the research described in the present volumes was developed by the initiative of individuals.

13. EXTENSION OF RESEARCH ABROAD

From time to time during the war requests for advice began to come in from some of the Colonies and other British-controlled territories abroad. *Rattus rattus* had been successfully controlled in the Port of London, and it seemed very likely that this—the chief plague rat—could also be surmounted by similar methods in these other countries. But it was essential to test them under actual conditions there, and relate them to the supplies of materials for baits. In 1945 Watson and Perry went out to the Middle East in order to make these tests under the auspices of the Colonial Office, but still on the Bureau's staff, paid by the A.R.C.

Palestine was chosen as the main scene of operations, and most of the tests were carried out in the Port of Haifa and on Jewish communal farms, a few also at Khartoum in the Sudan. Both species of rat occurred, but most of the work was done on *R. rattus*. The upshot of this year's work was satisfactory, and suitable wet cereal baits for use with arsenic and zinc phosphide were developed. The climate was mostly hot and dry, and its effects on the baits were studied rather carefully. They were reckoned to be suitable (climatically) for hot dry countries generally, and instructions were accordingly drafted (Ch. 14, Appendix).

The results of the Palestine and Sudan investigations could then be given to the war-time pest-control organizations in India and Burma. They were also taken up in Malta, where the British M.O.F. staged a very successful block control campaign in order to stop an outbreak of bubonic plague in the island (Barnett, 1948 *a*).

In 1947–8 Watson went to Cyprus for the Colonial Office to make a study of black rat control in the carob tree plantations. He was able not only to obtain a thorough picture of the biology and ecology of *R. rattus* field populations, and of the associated population of spiny mice (*Acomys dimidiatus*), but successfully carried out field poisoning tests that proved the practicability of rat control in carob-growing areas if only the human population was prepared to co-operate among itself and follow a properly organized campaign (Watson, 1951).

14. SPECIAL ACKNOWLEDGEMENTS

It would be an impossible task to acknowledge here all the assistance received during 8 years from hundreds of people, official and other. We are fully conscious how much they helped in the successful development of all this research and its application in practice. I will confine myself to mentioning the debt owed by the Bureau to a few whose support and guidance was vital; and to whom I, as organizer, remain particularly grateful: the late Sir Edwin J. Butler, the late Dr. W. W. C. Topley, and the late Sir John Fryer, successively Secretaries of the A.R.C., for their consistent and energetic support and wise counsel; Mr. E. H. E. Havelock, Administrative Secretary of the A.R.C. (now Secretary of the Development Commission), for his imperturbable pathfinding on our behalf through the jungle of the government departments and for understanding so well the tempo and atmosphere of research; the late Professor E. S. Goodrich for allowing the Bureau to have space in the Department of Zoology and Comparative Anatomy at Oxford and carry on its rather unorthodox activities there during and immediately after the war; Dr. R. E. Slade, Director of Research in Imperial Chemical Industries, for his encouragement at the beginning of the war, his insight into the difficulties of building up a system of attack on rodents, and for a most valuable piece of advice about dealing with government departments that I had better not repeat here; Dr. M. T. Morgan, Port Medical Officer of Health for London, not only for giving us the privilege of carrying out operational research for 4 years in the Port Area, but for his patience and understanding of the nature of our work, and his kindness to those younger members of our staff who worked with him.

Finally, I wish to make a special acknowledgement to the anonymous benefactor who generously enabled us to publish the present volumes. His gift of £2,000 was publicly acknowledged by Oxford University in a Decree passed by Congregation on 23 October 1951.

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2

PROPERTIES OF THE POISONS USED IN RODENT CONTROL

By R. B. FREEMAN

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Editor's note

The main account has been written by Freeman with the important exceptions of 'Methods of bioassay' (by Leslie and Thompson), 'Relationship between particle size and toxicity [of arsenious oxide]' (by Rzóska, Leslie, and Ranson) and 'Bioassays' in §§ 4 and 6 (by Leslie). In these investigations, particularly of red squill, a great deal of work was carried out by Elton. Freeman, however, prepared the account and did the research on squill extracts. The late R. M. Ranson was in charge of the rat stocks and carried out the toxicity tests up to 1944, after which the work was done by Thompson alone. Tests were planned in conjunction with Leslie who carried out all the statistical analyses.

1. GENERAL INTRODUCTION

(*a*) **Outline of treatment**

WORK by this Bureau on toxicological problems has varied from small observations, incidental to other work, to full-scale experiments such as those on standardization or danger to other animals. It has been considered most suitable to build these data into a series of general studies of each poison, containing not only what is new but also a survey of previously published work, giving as complete a picture as possible of the tools with which the field experimental studies were undertaken. Certain problems common to all poisons are discussed in this introduction; there is then a section on methods of carrying out toxicity tests, after which each poison has been treated separately. Although a large number of poisons has been recommended as suitable for killing rats and mice few have been used at all extensively and the amount of good information in the literature is very small. Only four poisons have been used consistently and extensively in this country during the war years: arsenious oxide, barium carbonate, zinc phosphide, and red squill (powder and extract). The greater part of the field experimental work has been carried out on one or other of these four, and they have therefore received the most extensive treatment here. Six further poisons have been treated fairly fully: phosphorus, thallium sulphate, sodium arsenite, sodium fluoroacetate, strychnine, and α-naphthylthiourea (Antu). On these poisons the Bureau has carried out little or no toxicological research, but they may have been used in field tests, and need to be considered because of their frequent use in this or other countries. A number of other poisons which have been recommended occasionally in the literature or which are of historic interest only are mentioned in this introduction.

Each of the important poisons is preceded by a summary of its salient characteristics: the popularity which any given poison possesses in Britain and other countries is based on correct or incorrect opinions derived from these characteristics. Without considering actual amounts used, or distribution of use amongst the various organizations responsible for killing rats, popularity is somewhat as follows. Zinc phosphide became a universal favourite during the war years; but arsenious oxide is a common poison, particularly in rural areas, where a strong tradition for its use is found. Barium carbonate has declined in popularity, especially since it is no longer officially advocated. There is still a strong demand for red squill as a safe poison for use where chickens and pets are present, but its general use has also declined. Other poisons have been used, if at all, only on a very small scale. Apart from the poisons advocated and used by official organizations, considerable amounts are sold as proprietaries or used by servicing companies. It is not intended to discuss the structure or efficiency of proprietary products but it is interesting to note what poisons are used. Phosphorus, barium carbonate, and red squill are undoubtedly the most frequent. Arsenious oxide, perhaps because of its scare reputation, is rare. The use of strychnine has been illegal since 1935, before which it was popular, but a brucine salt is still found in at least one proprietary product. Zinc phosphide occurs in several but is not yet as common as the others.

(*b*) History

Studied historically the poisons used in rodent control present certain points of interest. The earliest poisons used in this country, against field and house mice, before the advent of the brown or the black rats, were the vegetable poisons first used by the earlier civilizations of the Mediterranean basin—hyoscyamus, hellebore, henbane, houndstongue, hemlock, veratrum—in fact almost any plant that was known to be poisonous. The only well-known inorganic poisons were various arsenic compounds, and, although they were used very early for murder, they do not appear to have been used as mouse poisons. It is doubtful whether the strychnine that is used today was used then, for the strychnos of the Romans was not *Strychnos nux-vomica* from the Far East but probably some species of *Solanum*. The advent of the black rat did little to change this position; indeed, until the late seventeenth century few references can be found to the poisons used. Such authors as Markham (1631) and W. W. (1680) continue to refer to the same plant poisons. Red squill was advocated by Chomel in France (1718), before the arrival of the brown rat (*c.* 1750), and a translation of his work was available in Britain in 1725 when the brown rat had barely appeared; but there is no evidence that squill was used until the nineteenth century. Of the poisons still in use, arsenious oxide is the first to have been advocated in agricultural textbooks, and since it was so well known it is possible that it had been used considerably earlier; authors such as Worlidge (1669) recommended it in the seventeenth century and in general it has remained popular ever since. Genuine strychnine dates from about the same time. Phosphorus was first used in Germany and was introduced to this country by Carr in 1842; barium carbonate was also probably of German origin late in the nineteenth century but, although it was used in Britain earlier, its great popularity began in the First World War. In this century, too, the old plant products finally disappear from the textbooks. Zinc phosphide, apart from the American poisons of the Second World War, has been recommended for the shortest time. First used in 1911 by Grandi (1912) and by Ghetti (1912) to quell vole plagues in Italy, it only became widely known in the 1930's, but now has found great popularity. Thallium sulphate, again a German suggestion of the 1920's, has been widely used in the U.S.A. and its possessions but has not been taken up in Britain.

(*c*) **Choice of poison**

In deciding what poisons to use in any programme of research on improving the efficiency of rodent control, two possibilities are presented: either to use well-known poisons and to concentrate on improving the field techniques, or to test large numbers of substances known, or suspected, to be toxic in the hope of finding a panacea. If the first method is adopted almost the only laboratory toxicological studies required will be those that relate to standardization of the materials available by bioassay or chemical means; if the second a great deal of time and organization will be devoted to screening of products in the laboratory before any field tests can be undertaken. The work of the Bureau has been concentrated on the first method; no poison not previously used elsewhere has been tested.

A large number of factors influence the choice of what poison is suitable under a given set of circumstances, even within the range of the small number of theoretical chemical entities concerned. Source of supply under war-time conditions, and its corollary cost, may be of exclusive importance; thallium sulphate, though highly praised and much used in the U.S.A., could not be considered beyond laboratory scale tests because the peace-time sources of supply, Germany and the U.S.A., were cut off through import difficulties during the war and through very high cost. Barium carbonate was, however, easily available in unlimited quantities at low cost from the Northumberland mines and had therefore to be most seriously considered. Red squill, except for small prize cargoes of unknown antecedents, was not available for three years of the war. Sources of arsenious oxide varied so much that extensive standardization tests were necessary. Demands for other uses with higher priority also influenced supplies, particularly in the cases of phosphorus and arsenicals.

(*d*) Purity of samples

Much of the literature ignores the fact that commercial chemicals are seldom, if ever, constant in chemical and physical properties, either in different batches from the same source or in batches from different sources. Though the importance of these factors varies with the different substances, it must always be remembered that absolutely pure substances are usually unknown or available in very small quantities. In the case of plant products in which no attempt is made to extract the pure toxic agent the importance of batch variation is still further increased. Actual impurities present in any sample may influence its efficiency in two ways. If they are present in large quantities they may so reduce the toxicity that, at the concentration normally used for a purer sample, the rats may not eat enough poison bait to kill them. If they are present in small quantities the toxicity will not be seriously impaired, but even relatively small amounts may make baits unpalatable. If the amount of poison contained in a bait has to be increased because of the presence of impurities, refusal may result because of the mechanical grittiness, particularly in the case of substances of low toxicity such as barium carbonate. In practice it has been found that the presence of impurities is important in the case of arsenious oxide, and unimportant in the case of barium carbonate and zinc phosphide. Barium carbonate samples, whether native witherite or precipitated, have contained very low percentages of impurities; samples have been recorded however with large amounts of contained sulphate and more than 2% sulphide has been found deleterious (Kunhardt and Chitre, 1921). The principal impurity in zinc phosphide is excess zinc, but batches have remained very constant in field tests. The position with red squill is a little different; the toxic substance represents only a very small fraction of the whole and the rest of the plant matter can scarcely be classed as impurities. However, samples vary widely in their toxicity and the setting of a standard representing the available toxic content is essential.

(*e*) Other properties

(i) *Stability*

Stability of poisons or of poisons in baits may be an important factor. Arsenious oxide is in general very stable but it has been found that in certain baits chemical reaction may occur with the evolution in minute quantities of unpleasant smelling substances; failures in the field have been attributed to this. Zinc phosphide is unstable under moist or slightly acid conditions, and though it never becomes completely inert the toxicity may be considerably lowered. This deterioration has not been found important in this country, but under tropical conditions both lowered toxicity and decreased palatability have been found (Doty, 1945). The general phosphine smell, accentuated in wet baits, does not seem to decrease palatability. Phosphorus will deteriorate by oxidation in the same way and in this case the danger of fire is also present so that phosphorus baits must contain fat or syrup to surround the poison particles. Red squill powders will deteriorate on keeping, the deterioration being increased by moisture and light, and in consequence processes have been evolved which will keep the powder stable more or less indefinitely.

(ii) *Solubility*

Most of the poisons used are relatively insoluble in water, a character which presents various advantages and disadvantages. In mixing wet baits it is probably an advantage to use a soluble poison because a greater evenness of distribution can be obtained. In the case of phosphorus indeed it has usually been found necessary to use a suitable solvent, such as carbon disulphide, to ensure a very finely particulate mix which would otherwise be impossible to obtain. High solubility is however a disadvantage, particularly where baits have to be laid under very wet conditions; for instance, it has been found by Murray (1946) in tropical campaigns that sodium fluoroacetate leaches very easily from wet baits with consequent rapid loss of efficiency. Sodium fluoroacetate is now widely used in water solution in U.S.A. and other poisons have sometimes been recommended for use in this form. It has been suggested, particularly in the case of salts of strychnine, that relatively insoluble substances will be less bitter and therefore more freely accepted; and it is certainly true that substances arriving in the stomach in a dissolved state will be more quickly absorbed, giving rise to earlier symptoms and earlier death, a factor which is not necessarily advantageous.

Whether soluble in water or not, all poisons except mechanical ones such as plaster of Paris or glass, which are probably useless anyway, must be soluble in the secretions of the alimentary canal, usually in the acid secretion of the stomach. Consequently the size of the particles will affect the speed of absorption and therefore the time of onset of symptoms and death. It is also found that the actual toxicity will vary directly with the particle size, this probably relating to the amount that can be dissolved and then absorbed while the bait is passing the absorptive surface of the alimentary canal. Although this principle probably operates in the case of any insoluble poison it has been found particularly important in the case of arsenious oxide where there is a very wide range of toxicity and speed of onset of symptoms. Commercial samples vary greatly in particle size.

(iii) *Toxicity*

In general it may be said that, other factors being equal, the more toxic a poison the more likely it is to be successful in field campaigns. This is a wide statement and does not mean that relatively non-toxic substances such as barium carbonate will not kill field populations, but that the more toxic ones give good results more consistently. The reason is probably that in any given population there will be a small proportion of rats which will not feed freely on the bait at the particular time that it is offered to them. These rats, showing 'individual behaviour', are more likely to succumb to a small intake if the poison is very toxic, for though the percentage in the bait may be low the toxicity per gramme is usually considerable. A special study has been made of the onset of warning symptoms, with hesitation and subsequent refusal by a rat to take further bait (Ch. 8). It will suffice to say here that a poison which, by rapid absorption, gives warning to the rat that the bait is noxious, will be at a disadvantage over one where symptoms are delayed until the rat has consumed a lethal dose. Time of onset of symptoms does not necessarily relate to time taken to die. For instance, phosphorus may produce obvious symptoms quite quickly but death may be delayed for many days.

(iv) *Yield of corpses*

The speed with which death supervenes may have three bearings on the choice of a poison. Two of these, cruelty and increased danger to domestic animals, are discussed separately below. The third is that, to some extent, the quicker the death the larger the number of corpses that will be picked up. If a control campaign is carried out successfully and the rats are all, or almost all, killed there should be no necessity to show its efficiency by the production of corpses, whose number will at best represent only a small proportion of the total number killed (Ch. 4). Nevertheless, the sight of corpses does seem to encourage rat-catchers in their work and to give other people more concrete evidence than an assurance that their rats are dead. Campaigns carried out with zinc phosphide produce relatively many corpses, those with arsenious oxide very few. Antu, barium carbonate, and red squill, particularly perhaps the latter, produce some. Zinc phosphide is a quick-killing poison but the speed of death is not necessarily the only factor which makes the animals die in the open; they may go back to their burrows and re-emerge later to die. The reverse appears to be true with arsenious oxide, the rats tending to die deep down. With red squill death is often slow but is preceded by a long period of disorientated behaviour which may keep the animals in the open. In habitats occupied by man, a poison which produces most of its corpses at and just below the surface may be a disadvantage because of the resulting corpse smell.

(*f*) **Danger**

(i) *Man*

The problem of the danger to man and domestic or wild animals or birds is one that has received a great deal of attention in recent years. That to man falls under two heads, the danger of handling poisons and poison baits in the laboratory, and the danger that laid baits will be picked up in ignorance of their true nature. It is not within the scope of this paper to discuss the propriety of allowing dangerous poisons into public hands. In Britain, at least, all the poisons normally used in rodent control have limitations placed on their sale, or are so little known that they are unlikely to be demanded by untrained people; it will be assumed that the poisons themselves are being handled by trained or semi-trained staffs. Certain poisons such as arsenious oxide and barium carbonate present no dangers that cannot be eliminated by the ordinary common sense of keeping utensils clean, by using them exclusively for handling poisons and by careful personal hygiene. Other poisons require more detailed precautions: red squill powders should never be handled in bulk without a dust mask; zinc phosphide should be handled only in rooms with good ventilation and great care must be taken that staff do not remain in an atmosphere of phosphine, however dilute, for longer than necessary; care should be taken that soluble poisons are not absorbed through skin abrasions. The danger from accidental consumption of prepared baits either before or after laying seems to be very small even to children. In Britain during the twenty years 1919–38 it is recorded, in the tables of violent deaths in the *Registrar General's Statistical Review of England and Wales*, that eleven people died from the accidental consumption of rat poison; nine of these cases were from phosphorus and two from arsenious oxide poisoning. During the same period 105 suicidal deaths from rat poisons are recorded. These figures are obtained from the findings of coroners' inquests and it is possible that the source of the poison was not recorded in other accidental poisonings, but the figure is probably not far wrong. The chances are, then, of less than one person being killed accidentally each year by rodent poisons, a very much smaller chance than that of most of the preventable hazards of modern agriculture. Nevertheless every precaution should be taken to reduce the risk by careful labelling of poison bait containers and informing all persons likely to enter an area on which poison has been laid down. The addition of some bright colour not normally associated with food (blue has mostly been used) is a good precaution required by law in Britain for white arsenicals and in the U.S.A. for fluorides. It is perhaps interesting to note that of the eleven accidental deaths four are attributed to proprietary poison baits; such baits are often recommended for use on bread sandwiches or are prepared in the form of biscuits; these will be a much greater danger to children than loose bait piles.

(ii) *Animals*

Before considering the danger to animals, certain factors must be considered which make some poisons less dangerous than others. If a substance is poisonous to rats it will almost certainly be poisonous to all other mammals; it may be more or less poisonous within relatively small limits but will be to some extent harmful. Recent work on Antu appears to show that the range of toxicity of this substance is very wide, ranging from a few milligrams to several hundred per kilo for various mammals; but even so the upper limit is still distinctly toxic. Red squill is no exception to this rule though other safety factors are present. In the case of birds the position is different and it is clear that both red squill and strychnine are almost non-toxic to some species. It is frequently stated, or assumed, that the less toxic the substance the less the danger of using rat baits. It is obvious that this is true of the substances themselves, but it must be remembered that a less toxic substance will be used at a higher percentage in the bait so that this difference will be considerably ironed out.

The production of emesis by some poisons may act as a useful safety factor, though most ruminant animals as well as rats cannot vomit. In the case of red squill this will be a safeguard for man, cats, and dogs, but does not seem to save pigs and cannot save cattle and sheep. Barium carbonate does not usually produce vomiting, whereas arsenious oxide does, particularly if taken in large quantities. Indeed barium carbonate, in spite of a very low toxicity, has proved much more dangerous to cats than arsenious oxide. Unpalatability is an important safeguard with squill, which is intensely bitter to man and is usually strongly refused by domestic animals but apparently not by rats. Zinc phosphide similarly would not be taken unintentionally by man, but is readily accepted by chickens and pigs. Absolute size is a safeguard to the larger farm animals which will usually need to eat from more than a single small bait point to suffer ill effects. Slow acting poisons are safer, particularly to man, only if antidotal measures can be taken. Strychnine is forbidden for use in rodent control in Britain partly on the grounds that its action is so rapid that antidotes cannot be given in time; zinc phosphide is however almost equally quick and sodium fluoroacetate perhaps quicker.

The danger to animals also takes two forms, that due to eating prepared baits, and that due to eating dead or dying rats. During the war years considerable numbers of deaths have been recorded, particularly of poultry and domestic pets but also of larger animals. The total loss, however, is probably so greatly offset by the saving in foodstuffs from rats that its importance must not be unduly magnified. The first danger may be almost eliminated by consistently good and conscientious technique; the great majority of casualties, particularly those amongst poultry, is due to lack of experience or laziness in placing of baits. The danger to cats, and to a lesser extent to dogs and pigs, is from secondary poisoning. Cats should, of course, be shut up during a campaign, but this is not always possible, particularly on farms. The advantage of zinc phosphide in producing corpses in the open is considerably offset by the increased danger to cats. It spoils the effect of showing the kill to the farmer if his cats are included.

Suggestions have been put forward from time to time that more stress should be laid on finding methods of rat destruction which involve the least cruelty. Any constructive ideas put forward have usually been based uncritically on a supposition that what is cruel to man, by prolonging pain unnecessarily, will be cruel in the same ratio to rats; or on an interpretation of observed symptoms in anthropomorphic terms. While not considering these premises necessarily valid, it seems reasonable, other things being equal, to choose poisons which bring about fairly rapid death with a minimal apparent agony. Red squill, from interpretation of observed symptoms, is a cruel poison; death only supervenes after several days and the symptoms are unpleasant. Strychnine acts quickly but the convulsions characteristic of its action are often violent. Zinc phosphide on the other hand, though it acts quickly, produces almost no signs of distress until the short death agony.

2. METHODS OF BIOASSAY

(*a*) Introduction

Chemical assays of poisons are relatively straightforward and give very consistent results, but they are not true indices of physiological activity. Physical factors, such as particle size in arsenic and specific rotation in strychnine, must be taken into account and even when all these have been standardized, variations in physiological activity have been observed (Munch, Garlough, and Ward, 1936). In order to assess the toxicity of a poison sample it is thus necessary to conduct bioassays, i.e. toxicity tests upon live animals. The general principles which are to be observed in carrying out such tests, and the type of result which is obtained, are well known and need not be detailed here. In particular, reference may be made to Gaddum (1933), Bliss (1935 *a* and *b*), Irwin (1937) and Finney (1947), to quote only a few from among the many papers which are to be found in the literature. The actual methods used in carrying out the toxicity tests were as follows; the results are given later.

(*b*) Experimental methods

All the standard bioassays have been done upon Wistar strain albino rats (*Rattus norvegicus*) inbred for some years at Oxford and by the Bureau since

1939. Rats from an inbred stock, having a more homogeneous genetic constitution, are much more alike in their resistance than any mixed stock of wild rats (see Gaddum, 1933), and their reaction at different times towards a given poison sample is more consistent. Similar stocks kept in different laboratories may, however, show marked differences (e.g. Munch *et al.*, 1937). Poison may be given to animals by injection, by stomach tube, or by the mouth, and this last method was adopted on the grounds that it approached most nearly to field conditions.

Sex and age differences in the resistance of a species towards some poisons are known to exist. We have, however, made no attempt to measure such differences in the case of all the poisons which have been tested in this work. So far as was possible only animals of a similar age and weight and of the same sex were used for testing the toxicity of different samples of a particular poison. Thus it was evident quite early in our work that a large number of tests would have to be carried out on red squill. The evidence in the literature, which we have checked by a recalculation of the published data, is that females are significantly more susceptible than males to the toxic action of this substance (Crabtree, Ward, and Welch, 1939; Winton, 1927: Professor F. R. Winton kindly supplied details not available in the paper itself). In consequence, male white rats only were used in toxicity tests upon red squill.

Since the number of animals available at any time was limited, and because the males were reserved for the tests of squill, females were used for those on arsenious oxide, the substance which next to squill, was the most frequently tested. In regard to the remaining poisons, on which only a limited number of tests were done, male rats were used for zinc phosphide and barium carbonate, females for Antu. As far as possible only rats from 4 to 6 months old and between 150–250 g. in weight were used in all these experiments.

Finally, a standard bioassay technique was evolved as follows. If little was known regarding the toxicity of a substance, a preliminary test was done using four or five groups each of five animals. The individuals in each group received the appropriate dose of poison, which was calculated per kilo body weight. In these preliminary tests the doses per group were chosen so as to cover a relatively wide range of toxicity. The results of such a test (e.g. 0/5, 1/5, 3/5, 5/5 rats dying at the different dose levels) were then used to calculate dose levels for a full test in which from three to six groups of ten to twenty animals each were used.

On the first day of the experiment the rats, not previously used in any other test, were selected and placed in individual cages without litter. Each was given a surplus (40 g.) of flour and water paste (two parts national flour to one part water). After 24 hours, all bait residues were removed from the cages and the rats starved for 24 hours. On the third day the rats were weighed individually and apportioned to the different dose levels so that there was an equal biomass (total weight) of rats at each dose. Each poison dose was weighed out upon a chemical balance to the nearest tenth of a milligram, and placed upon a numbered watchglass where it was thoroughly mixed with a pellet of flour and water paste (weighing from 1–1·5 g. according to the size of the poison dose). The poison pellet was left in the cage with the rat for 24 hours, and if it was wholly or partially uneaten at the end of this period that rat was discarded from the test. Normal food was placed in the cages on the fourth day, and all deaths up to 4 days later were attributed to the poison. The rats were provided with a surplus of water throughout the experiment.

The rats were denied litter during the test because it has been found that white rats have a habit of eating their litter (and also their own dung) soon after they have eaten a dose of poison. It is possible that this is a reflex action to the stimulus of stomach ache, and that the ingested litter might act as a partial antidote (Chs. 8 and 9).

After prebaiting and starvation very few rats refuse the poison bait. Observation, in the laboratory and the field, has shown that rats taking sublethal doses of poison usually recover in 1–4 days and take normal amounts of food. Conversely, those that have taken lethal doses usually die within 5 days of taking the poison. Only in the case of arsenic of very large particle size have test animals been known to suffer very delayed deaths—up to 12 days. Water is given throughout the test in order that the rats may retain their normal appetite.

In the case of squill extracts, the standard technique was modified as follows: 24 hours' prebaiting with a surplus (120 g.) of 2-g. cubes of day old national bread, soaked in their own weight of water, was followed by 24 hours' starvation. The poison, in suitable dilution, was measured out with a ml. pipette and each does was added to a 2-g. bread cube.

(*c*) Statistical methods

The statistical methods used in the analysis of the data are fully described by Bliss (1935, *a* and *b*), Irwin (1937), Finney (1947), and in the introduction to Fisher and Yates (1943), the percentage mortalities being transformed to probits and the logarithm of the dose taken as the independent variable. Convenient tables of probits and of the weighting coefficients which are required in the actual numerical process of fitting the straight line, will be found in Fisher and Yates (1943, tables 9 and 11.) In most of the tests discussed later a provisional regression line was fitted without weighting to the probits available, and from this line the working probits were obtained for a first approximation. This first approximation was then used as the basis of a second, the process usually terminating at this stage, though occasionally a third approximation was found to be necessary.

The equation relating the working probits (y), each with a weight w, to the logarithms of the doses (x) is

$$y = \bar{y}+b(x-\bar{x}),$$

where $\bar{y} = \dfrac{Swy}{Sw}$, $\bar{x} = \dfrac{Swx}{Sw}$, and $b = \dfrac{Swy(x-\bar{x})}{Sw(x-\bar{x})^2}$.

For testing the goodness of fit of the regression line, we have

$$\chi^2 = Sw(y-\bar{y})^2 - \frac{[Swy(x-\bar{x})]^2}{Sw(x-\bar{x})^2},$$

the number of degrees of freedom being two less than the number of points used in fitting the line. Provided that this goodness of fit test is satisfactory, the theoretical forms of the variances of $\bar{y}$ and b may be used, namely,

$$V\bar{y} = \frac{1}{Sw}, \qquad Vb = \frac{1}{Sw(x-\bar{x})^2}.$$

But if χ^2 is excessive, and there is no obvious reason for abandoning the hypothesis of a linear relationship between y and x, each of these estimates of the variance is multiplied by χ^2/n, where n is the number of degrees of freedom.

The logarithm of the dose which will on the average kill 50% of the test animals (L.D. 50), corresponding to a probit of 5, is

$$x = \bar{x}+\frac{5-\bar{y}}{b},$$

and in cases where the goodness of fit test is satisfactory, the variance of this estimate may be taken as

$$s^2 = \frac{1}{b^2}\{V\bar{y}+(x-\bar{x})^2 Vb\}.$$

It is convenient to give some indication of the range of error attached to the L.D. 50. We may choose some level of probability such as $P = 0{\cdot}95$ or $P = 0{\cdot}99$, and say that in the long run the true value might be expected to lie within the limits antilog $(x \pm ts)$ in 95 or 99 experiments out of 100. When a highly significant value of b has been found, this method gives a satisfactory estimate of the range. But in a large number of cases met with in practice, where the slope of the regression line may be estimated with only a moderate degree of precision, it is known to give only approximate results which may seriously underestimate the true limits. For a detailed discussion of this point and examples reference should be made to Irwin (1943) and Finney (1947). A more exact determination of the range may be made from the equation

$$x = \bar{x}+\frac{b(5-\bar{y})}{b^2-t^2Vb} \pm \frac{t\sqrt{\{Vb(5-\bar{y})^2+V\bar{y}(b^2-t^2Vb)\}}}{b^2-t^2Vb},$$

and taking the antilogarithms of the two values thus obtained. When the goodness of fit test for the regression line is satisfactory and the theoretical form of the variances of b and $\bar{y}$, given above, are used, the value of t is taken as 1·96 for a range corresponding to $P = 0{\cdot}95$, or as 2·576 for $P = 0{\cdot}99$. If, however, χ^2 is excessive, and these variances are each multiplied by χ^2/n, the value of t is taken from the appropriate table for the number of degrees of freedom left after fitting the regression line ($N-2$). Since in all the individual tests which will be given later, satisfactory values of χ^2 were found, t was taken as 1·96, and the exact method of determining the range was used throughout.

In comparing any two regression lines for significant differences either in position or slope, the methods suggested by Bliss (1935 *b*) have been used. If in the case of the first line we put

$$Sw_1 y_1(x_1-\bar{x}_1) = B_1, \qquad Sw_1(x_1-\bar{x}_1)^2 = A_1,$$

and, similarly, B_2 and A_2 for the second line, the slope of the best fitting pair of parallel straight lines is given by

$$b_c = \frac{B_1+B_2}{A_1+A_2}.$$

Then for testing for differences in position (one degree of freedom),

$$\chi^2_{\bar{y}} = \frac{[(\bar{y}_1-\bar{y}_2)-b_c(\bar{x}_1-\bar{x}_2)]^2}{V\bar{y}_1+V\bar{y}_2+\dfrac{(\bar{x}_1-\bar{x}_2)^2}{A_1+A_2}};$$

and for differences in slope (one degree of freedom),

$$\chi^2_b = \frac{(b_1-b_2)^2}{Vb_1+Vb_2}.$$

Occasionally it is necessary to compare a number of estimates of the slope. If the common slope (b_c) be calculated by an extension of the equation given above for a pair of lines,

$$\chi^2 = \sum_p A_p(b_p-b_c)^2:$$

which from the computational point of view is better written as

$$\chi^2 = \sum_p B_p b_p - b_c \sum B,$$

the number of degrees of freedom being one less than the number of slopes compared.

In a number of tests it may be of greater interest to compare the relative potency of two samples, such as some standard preparation and an unknown, than to measure the actual value of the L.D. 50. Then, assuming that there is no significant difference between the slopes of the separate lines and that a pair of parallel lines give a satisfactory fit to the data, if $\bar{x}_1$ and $\bar{y}_1$ are the means for the standard preparation and $\bar{x}_2$ and $\bar{y}_2$ those of the unknown,

$$\log\left(\frac{\text{Potency Unknown}}{\text{Potency Standard}}\right)=\bar{x}_1-\bar{x}_2+\frac{(\bar{y}_2-\bar{y}_1)}{b_c}.$$

The variance of this estimate may be taken as

$$s^2=\frac{1}{b_c^2}\left(V\bar{y}_1+V\bar{y}_2+\frac{(\bar{y}_2-\bar{y}_1)^2}{b_c^2(A_1+A_2)}\right);$$

and, according to Irwin (1943), the exact limits of the range of error by:

$$\bar{x}_1-\bar{x}_2+\frac{b_c(\bar{y}_2-\bar{y}_1)}{b_c^2-t^2Vb_c}\pm\frac{t\sqrt{\{Vb_c(\bar{y}_2-\bar{y}_1)^2+(V\bar{y}_1+V\bar{y}_2)(b_c^2-t^2Vb_c)\}}}{b_c^2-t^2Vb_c}.$$

(*d*) **Variations in susceptibility**

(i) *Design of the experiments*

A note should be added in regard to the design of these experiments, which to some extent runs counter to the generally accepted practice. Undoubtedly the most satisfactory way of carrying out comparative toxicity estimations is to test a standard sample of the poison and the unknown at one and the same time. In this way the complicating factors due to any day to day variation in the response of the animal stock towards the particular agent can be eliminated. Our initial tests, however, were upon squill powder and we were uncertain whether it would remain stable under the conditions of storage. If any marked deterioration were to occur, it was clear that considerable difficulties might be met with in maintaining a satisfactory standard for the comparative tests. But a more decisive factor in regard to the general design of the experiments was the realization at an early date that the supply of rats of the right age and sex was likely to be on the short side, and that time and labour would be limited to some extent by the demands of other work. Under these conditions it seemed best to devote such animals as were available to the determination of the dosage-mortality curve for each poison sample which showed signs of being reasonably toxic. This procedure would be satisfactory provided it were assumed that any variations in the response of the rat stock to a particular poison could be neglected in comparison with the differences between samples which would be important from the point of view of field work. In order to check this point, it was decided to carry out repeat tests whenever a sufficient number of animals had accumulated in the stock. In this way it was hoped that a body of information would be gradually built up in regard to the response of the animals to the principal poisons which were tested.

(ii) *Response of the stock in 1942–4*

In general it does appear that over a considerable period of time the rat stock used did give very consistent results. This may be seen from the results of the tests with the standard samples of red squill and arsenic. Thus, as examples, we may take the five tests done between February 1943 and July 1944 with the S 1 sample of squill (T. [= Test] 20, 21, 41, 43, and 49)[1] and the five tests on arsenic sample 2 between March 1942 and February 1943 (T. 01, 02, 9, 16, and 18). These tests give respectively fifteen points for the determination of the squill dosage mortality curve and fourteen points for that of the arsenic. A single regression line was fitted to each series and tested for goodness of fit. The values of χ^2 were 11·96 (13 d.f.) for the squill and 13·94 (12 d.f.) for the arsenic tests. Both of these values are reasonable, $P = <0{\cdot}70>0{\cdot}50$ and $P = <0{\cdot}50>0{\cdot}30$ respectively, and thus a single regression line is a satisfactory fit in each case. These results strongly suggest that allowing for the scale on which the individual tests were carried out, there was no very marked variability in the response of the stock to these particular poisons.

(iii) *Susceptibility after July 1944*

Events in 1944.—Although consistent results were thus obtained with these two standard samples over a considerable period of time, a change in the animal stock undoubtedly did occur during the summer and autumn of 1944. Murine scabies (*Notoedres muris*) was accidentally introduced into the animal house in August 1943 and during the spring and summer of 1944 this infection became suddenly acute, spreading widely throughout the stock and leading to a good deal of trouble, in particular to a greatly increased infantile mortality. During the process of clearing up the infection, certain changes in the diet were introduced, consisting principally in the addition of green vegetables, cod-liver oil, and whole wheat. From the somewhat anomalous results of a few toxicity tests which were made during the late autumn of 1944, it was suspected that some change in the susceptibility of the animals had taken place. Consequently, when the stock had recovered in numbers, further tests with the same standard samples of squill and arsenic were carried out.

Red squill.—As regards squill, seven tests were made using sample S 1 between 1 December 1944 and 2 March 1945 (T. 58, 61, 62, 63, 64, 65, and 73) which gave twenty-three points on the dosage mortality curve. Fitting, as before, a single line to these data, the value of $\chi^2 = 46{\cdot}7$, which is excessive for 21 degrees of freedom, $P = 0{\cdot}001$ approximately. A single line was thus not a satisfactory fit. The main trouble was apparently caused by the results of the test carried out on 1 December 1944 (T. 58). This gave 100% mortality at a dose of 300 mg. per kilo, 80% at 400, 100% at 600, and 100% at 800. Whereas this degree of mortality at the higher dose levels did not appear excessive as judged by previous experience with this sample, the 100% mortality at 300 mg. per kilo was entirely unexpected. If this test is eliminated, χ^2 for the remaining six tests is 21·62, and for 17 degrees of freedom $P = 0{\cdot}20$, which is quite a reasonable value. Thus the results of the tests with this sample of squill were apparently consistent during the period 15 December 1944–2 March 1945. The L.D. 50 estimated from this latter line (neglecting the test on 1 December) is 395 mg. per kilo with a range ($P = 0{\cdot}95$) of 354–446, and that given by the single line fitted to the earlier data for this sample of squill was 443 mg. with a range of 380–504. Testing for any differences between these two lines, $\chi^2 = 1{\cdot}53$ for differences in position and $\chi^2 = 0{\cdot}28$ for differences in slope. Neither of these values is significant. Although the L.D. 50 was somewhat lower when the stock had settled down after the upset during the summer of 1944, the difference from the results of the early tests was not very marked. It is however difficult to justify the complete omission of the test made on 1 December. Slightly aberrant though it may be, it does contribute a certain amount of information as to the reaction of the animal stock under the changed conditions. Including this test in the analysis therefore, and allowing for the excessive value of χ^2 in estimating the variances of $\bar{y}$ and b, the L.D. 50 is 357 mg. per kilo, with a range of 285–418 mg., a figure lower still than that given above for the new series of tests on this sample. Since the slopes of the two regression lines did not differ significantly, an estimate was made of the ratio 'New tests/Old tests' and this was found to be 1·235 with a range ($P = 0{\cdot}95$) of 0·981–1·560. Since the value of unity for the ratio is included in the range, it cannot be stated with certainty on these results alone that the stock had become more susceptible to red squill. It is, however, suggestive that a similar type of shift in the direction of a lower L.D. 50 was also observed in the case of arsenic.

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[1] Full details of these tests are deposited; analyses are in Tables 1 (p. 45) and 10 (p. 76).

Arsenic.—Between 29 January and 14 August 1945 five tests (T. 68, 70, 74, 76, and 86) were carried out with arsenic sample 2. Fitting in the same way a single line to these 19 points, $\chi^2 = 23{\cdot}9$ and for 17 degrees of freedom $P = <0{\cdot}20>0{\cdot}10$, so that again the results appear to have been satisfactory over a period of about 7 months. However, the L.D. 50 during this time was 89 mg. per kilo with a range ($P = 0{\cdot}95$) of 84–95 mg. while that for the original series of tests with this sample during 1942–3 was 115 mg. per kilo with a range of 106–124 mg. There can be no question as to the significance of the difference in position of these two lines; there was, however, no significant difference between the slopes. Thus it seems clear that the rat stock as a whole had become more susceptible to arsenic in 1945 than it had been during 1942–3. The log ratio (1945/1942–3) was $0{\cdot}1134 \pm 0{\cdot}0215$, which gives a ratio of 1·298 or an increase of about 30%. This shift in the general level of susceptibility towards arsenic is confirmed by a comparison which can be made between two series of tests on arsenic sample 5, one test done on 5 September 1942 (T. 5) and the others (T. 59

and 60) on 7 and 13 December 1944. The log ratio (1944/1942) is 0·1233 ± 0·0421, or a ratio of 1·328, a value obviously very similar to that given above for arsenic 2. As before, the difference between the regression lines is in regard to their position, and not in regard to their slopes.

The principal conclusion to be drawn from the analysis of these data is that consistent results were obtained with arsenic and squill during 1942, 1943, and the early part of 1944. This run of successful results was then interrupted by the upset to the rat stock caused by the spread of an infection during the summer of 1944. When this infection had been eliminated and the stock had settled down under certain changes in the diet, relatively consistent results were again obtained after 1 December 1944, though a shift of the L.D. 50 towards the lower dose levels was noted, particularly in the case of arsenic. A great deal of this apparent consistency must depend, however, upon the scale on which the individual tests were done, usually ten rats to each dose level and about 30–60 animals used in a full test. If each of these tests had been done on, say, five or ten times the number of animals actually used, it is possible that statistically significant day to day variations in the position of the L.D. 50 might have been observed similar to those which have been reported by other workers (e.g. Bliss, 1940). But in regard to the results which are given later, it does not seem as if any very great error would be made in comparing any two samples of a particular agent provided that the tests were carried out roughly about the same time, since the differences between samples which are of importance in the discussions of the individual poisons are for the most part relatively great. It is therefore necessary to distinguish between the old series of toxicity tests carried out previous to the summer of 1944 and the new series carried out after the autumn of 1944, and this has been done in all the comparisons which are made later.

3. ARSENIOUS OXIDE

(*a*) Introduction

Arsenious oxide has been used extensively during the war years by the rat killing organizations of the M.A.F. and the M.O.F. It may be considered a thoroughly efficient poison provided that suitable baiting techniques are used and the poison-sample itself is reliable.

Arsenical substances are amongst the earliest known poisons and there is a large literature on their use as murderers' tools (Blyth and Blyth, 1920). There is little evidence, however, that their use as rat poisons was at all extensive until the sixteenth century. Rogers, in Barrett-Hamilton (1910–21), states that arsenic was used in Britain in the fourteenth century when the black rat was only beginning to be a widespread nuisance, and the details of a bait formula are given in 'Le ménagier de Paris', a late fourteenth-century manuscript on household management.

The word arsenic originally referred to the yellow trisulphide orpiment but was later used for the red bisulphide realgar, and early references to the use of arsenic as rat poison probably refer to the latter substance. Thus Percivall in his Spanish dictionary (1591) gives '*rejalgar*, poison, arsenicke, or ratesbane' and Thynne (1599) [see *Oxford English Dictionary*, Arsenic] states 'this resalgar is that which by some is called ratisbane, a kind of poyson named arsenicke'. The word ratsbane, meaning merely rat poison (Old English *bana*, a slayer), has probably been used for any substance suitable for poisoning rats, but is more specifically confined to arsenic; and four references of this use in the sixteenth century, the earliest in 1523, are given in the *Oxford English Dictionary*; the same term used by Shakespeare in *Measure for Measure*, I. ii, perhaps also refers to arsenic.

The first certain use of the word to mean the trioxide is in 1605, when it was manufactured. From then onwards the trioxide was the substance normally used for rat killing, and characteristic formulae may be found in Worlidge (1669) or Chomel (1718); other writers of the period, such as Markham (1631) or Mortimer (1707), however, ignore arsenic and mention only the poisons in earlier use. After the invasion of the brown rat early in the eighteenth century, recommendations for the construction of poison baits containing arsenic are given in many of the agricultural textbooks. Some of these works are very interesting in that they contain methods and observations which are still of importance. Simpson (1777) refers to the fact that rats poisoned with arsenic drink water freely, and two other early textbooks entirely devoted to rat killing, Smith (1768) and Swaine (1783), both refer to the use of arsenic, though Smith did not consider it efficient because of its sharp taste. Meyer (1817) and Sinclair (1821) both recommend prebaiting in a manner very similar to that used in East Anglia today.

In this century arsenious oxide has decreased in favour considerably until its adoption by official rat control organizations during the war. This decrease may in part be due to the tightening up of legislation against the free sale of arsenicals, which favours rat baits containing barium carbonate and phosphorus. It may also be due to its scare reputation; from popular fiction and from history everyone knows that arsenicals are poisonous and they are perhaps cloaked with a greater danger than they really possess. Very few branded rat poisons contain arsenious oxide and it has not been widely used by servicing companies or by local authorities. There was, however, an extensive use of it by gamekeepers and individual professional rat-catchers in agricultural areas, particularly those in which game was strongly preserved such as Norfolk and Hampshire.

(*b*) Physical and chemical properties

Arsenious oxide, 'arsenious acid', white arsenic, 'arsenic', molecular formula As_2O_3, in a chemically pure state is a white, more or less finely divided powder. When formed as a sublimate it is in the form of shining white octahedral crystals; in commercial manufacture, however, it contains a certain amount of vitreous and amorphous particles. So called 'colloidal arsenic' may be formed by grinding fine arsenious oxide with a suitable colloid such as gum arabic. The solubility in water is very low but varies widely with the composition of the sample. Taylor's (1948) statement that an ounce of cold water dissolves from half a grain to a grain probably covers the range of variation, i.e. 0·1–0·2 parts in 100. It is slightly more soluble in dilute acids. With alkalis, dilute or strong, arsenious oxide goes into solution with the formation of arsenites.

It is stable in air and normally stable in made up rat baits, but wheat grains coated with pure starch and dusted with arsenious oxide quickly develop an unpleasant smell which makes them unpalatable. Broken cereals such as middlings and kibbled wheat, without added starch, also develop a similar smell with arsenious oxide, but more slowly. Sugar-meal baits apparently do not develop this smell with arsenious oxide of medium particle size (Ch. 4). Pure samples are without smell to man; they have a gritty and to some palates very slightly acid taste.

Commercial samples vary widely in their composition. The success or failure of arsenious oxide as a poison for rats depends, perhaps more than in any other of the commonly used poisons, on the absence of rat-deterrent impurities and on range of size and physical nature of the particles in any given sample. Apart from those which had been artificially coloured, twenty-six commercial samples received during 1939–45 varied in colour from grey to white, a few being pink or buff. The refined samples are usually more or less white, the crude ones grey or dirty white from impurities, usually sulphides. The general range of particle size is visible to the naked eye; very coarse samples will run like salt, and very fine will pack like flour even when the moisture content is low. Some, including refined samples, have a bitter or acid taste, much stronger than that of the pure substance. These differences will affect not only the toxicity of the samples, but also the acceptability of the bait in the field. Toxicity varies inversely with the particle size (or perhaps more accurately with the speed of solution, which will depend both on the surface-volume ratio and the physical state of the particles) and directly with the amount of arsenious oxide present. Except in very adulterated samples this latter point is unimportant. Acceptability depends on the nature and amount of the impurities. We are indebted to the Government Chemist for analysing various samples. The problem of particle size is considered below.

During the war it was not possible to lay down a standard for use in rodent control but samples had to be tested for suitability as they became available. Chemical details of nine of these samples, ones which were also used in toxicity and field tests are as follows: In chemical composition they may be compared with the 'Analar' standard laid down:

	%
Arsenious oxide	99·9985
Sulphide	no reaction
Heavy metals (lead)	0·001
Iron (Fe)	0·0005
Antimony, cadmium, tin	no reaction

Sample 2. Refined from U.S.A. 1941. White. Slightly acid taste. No smell.

	%
Arsenious oxide	97·5
Lead	0·3
Tin	0·01
Antimony	0·10
? Arsenic in higher state of oxidation	2·09

Particle size (Table 3) ranging from 3–36 with median particle size 4·5μ; 90·6% particles $<10\mu$. A very good fine-particle sample with a L.D. 50 at 115 mg. per kilo (Table 1). This sample was used as a standard in toxicity tests by which all other samples were compared.

Sample 12. Crude, from Canada 1942. Dirty pink but not artificially coloured. Unpleasant slightly acid smell and a very acid taste; has a numbing effect on the mouth and tongue.

	%
Arsenious oxide	73·3
SO_3	6–10
Fe_2O_3	3–5
Insol.	6–10
CaO and MgO	1
Gold	0·06–0·08 oz. per ton
H_2O	balance

The acidity of the sample is largely due to the high SO_3 content. The product is an extremely fine powder, of similar range to Sample 2. The toxicity test gave a L.D. 50 of 112 mg. per kilo; but if this figure is adjusted for the 26·7% impurity this is the most toxic sample studied, with a L.D. 50 of 82 mg. per kilo.

Sample 5. Refined, from Canada 1942. This sample was made by refining sample 12. Pale grey, very lightly artificially coloured. Slightly acid taste and smell.

	%
Arsenious oxide	94·5
Moisture	0·5
Residue on ignition	1·0
Balance	4·0

The balance is colouring matter and presumably arsenic in the higher state of oxidation. The 'ash' consists mainly of sodium compounds, the amount of other elements, e.g. lead, calcium, tin, magnesium, aluminium, and titanium being quite small. Manganese, zinc, strontium, and barium were negligible or non-detectable. The product was much coarser than the crude

TABLE I. *Summary of toxicity tests of arsenious oxide*

Arsenic sample No.	*Median particle size (μ)*	*Test number*	*Sex*	*Slope of regression line (b)*	*log L.D. 50*	*L.D. 50 (mg. per kilo)*	*Range % (P = 0·95)*	*Adjusted L.D. 50 (mg. per kilo)*
2	4·5	T. 00	♂	15·404 ± 4·412	1·9896 ± 0·0216	98	86–110	122
2	..	T. 01, 02, 9, 16, 18	♀	6·941 ± 1·025	2·0611 ± 0·0171	115	92–108	115
2	..	T. 68, 70, 74, 76, 86	♀	8·599 ± 1·168	1·9517 ± 0·0136	89	93–106	117
12	?	T. 08, 09	♀	11·249 ± 3·302	2·0492 ± 0·0254	112	81–110	82
5	10·0	T. 5	♀	8·007 ± 1·912	2·2703 ± 0·0302	186	84–115	186
5	..	T. 59, 60	♀	6·587 ± 1·542	2·1436 ± 0·0278	139	87–115	182
31	8·7	T. 87	♀	14·824 ± 3·701	2·0834 ± 0·0138	121	93–107	158
20	6·5	T. 6, 7, 9	♂+♀	4·443 ± 0·780	2·2512 ± 0·0316	178	87–118	..
20	..	T. 7, 9	♀	3·832 ± 1·551	2·2344 ± 0·0542	172	44–122	172
7	25·3	T. 001 (after 5 days)	♂	4·863 ± 1·640	2·5236 ± 0·0467	334	76–133	416
7	..	T. 001 (after 8 days)	♂	4·256 ± 1·622	2·4428 ± 0·0576	277	56–131	346

Arsenic sample No.	*Median particle size (μ)*	*Test number*	*Sex*	*Comparative test of standard*	$\log\left(\frac{\text{potency sample}}{\text{potency standard}}\right)$	*Ratio potencies*	*Range % (P = 0·95)*	*Adjusted L.D. 50 (mg. per kilo)*	*Range % (P = 0·95)*
26	10·1	T. 38	♀	T. 01, 02, 9, 16, 18	$\bar{1}$·7507 ± 0·0502	0·563	78–126	204	80–128
23	5·6	T. 17	♂	T. 00	$\bar{1}$·6986 ± 0·1048	0·500	30–163	230	61–336
21	16·1	T. 14	♂	T. 00	$\bar{1}$·6959 ± 0·0402	0·497	77–117	231	86–129

In the last columns the L.D. 50 is adjusted to allow for the time at which the test was done, the sex of the animals used, and, in the case of a crude arsenic, the percentage of arsenious oxide in the sample. All the results are thus expressed in terms of the L.D. 50 of the standard arsenic 2 tested on female rats during 1942–3 (115 mg. per kilo). In the first part of the table the percentage range of error of the observed L.D. 50's can be taken as applying also to the adjusted values.

from which it was derived. Median particle size 10·0μ; 51·5% particles <10μ. Sample 5 was considered a typical 'medium' sample and was used extensively in field tests. The L.D. 50 was higher than that of the crude sample, being 186 mg. per kilo.

Sample 20. Refined from U.S.S.R. 1942. Light grey. Strong smell suggestive of pine oil; no taste.

	%
Arsenious oxide	93·0
Moisture	0·3
Residue on ignition	3·1
Balance	3·6

The balance is presumably arsenic, in the higher state of oxidation. The 'ash' is mainly aluminium and silicon with fair amounts of antimony and iron. Lead, calcium, magnesium, and titanium were present in small amounts, while manganese, zinc, tin, strontium, barium, and sodium were negligible. The particle size was fairly fine. Median particle size 6·5μ; 74·8% particles <10μ; L.D. 50 of 172 mg. per kilo.

Sample 26. Refined from U.S.A. 1943. White. No taste or smell.

	%
Arsenious oxide	99·4
Antimony (as Sb_2O_3)	0·6

This sample is free from the number of small impurities present in the others and has a very high arsenic content. The particle size was fairly coarse. Median particle size 10·1μ; 50·3% particles <10μ. These two figures are very close to those for sample 5, but the actual distributions of particle sizes differ slightly in one or two respects. Sample 26 has a larger proportion of very fine and medium particles and very large particles are deficient as compared with sample 5: the latter feels slightly more gritty when rubbed between the finger and thumb.

Sample 31 was an uncoloured specimen of sample 5, having the same physical properties. *Sample 7*, probably of Mexican origin, was very coarse and coloured, having a median particle size of 25·3μ. The L.D. 50 in a standard test, counting only those animals which died during the first 5 days, was 416 mg. per kilo: however, the action was so slow that animals continued to die up to the eighth day and if these are included the L.D. 50 was 346 mg. per kilo. *Sample 23* was crude from U.S.A., having a median particle size of 5·6μ and a L.D. 50 of 230 mg. per kilo. *Sample 21* was refined Cornish, having a median particle size of 16·1μ and a L.D. 50 of 231 mg. per kilo.

(*c*) **Relationship between particle size and toxicity**

In the course of some experiments which were made for an entirely different purpose, it was observed that all the rats receiving a dose of 200 mg. per kilo of a fine-texture arsenic died, whereas those receiving a similar dose of a coarse-texture arsenic were not even seriously ill. On following up this chance observation it appeared that it was necessary to administer roughly up to three times the amount of this coarse arsenic in order to obtain a degree of mortality equivalent to that given by the fine. Moreover, further experiments showed that the time to death might be delayed for any time up to 8 or 9 days after ingestion of the coarse type, whereas in the case of fine or medium-coarse samples all deaths usually occurred before the fifth day. It was also found that the coarse arsenic became highly lethal at doses of 200 mg. per kilo after being ground by hand with a pestle and mortar to a state of great fineness. The samples used in these experiments were Nos. 2 and 7. At this time we were unaware that that differences in toxicity between arsenics of varying particle size had been pointed out by Schwartze (1922) and in further work by Schwartze and Munch (1926).

Since a fine grade of arsenious oxide powder had been used in all our early field trials, it seemed that some quick method was needed for estimating the texture of different samples in the event of a failure in the existing supplies; also that the relationship between particle size and toxicity should be investigated more fully. Attempts to measure the texture by means of sieving were abandoned—the finest mesh size used (300 per in.) being too coarse to determine the percentage of very fine particles—and a microscopical method was adopted for measuring the crystal size.

(i) *Technique*

A small amount of the sample is distributed as evenly as possible in a drop of oil (we have used ground-nut oil) on a slide. A cover slip is then placed on the arsenic-oil suspension and by a gentle pressure of the fingers, aided by a slight rotary action, the drop is spread out evenly under the cover slip. The preparation is then examined microscopically (objective $\frac{1}{6}$ in.; micrometer eye-piece ×2) and a field found where the majority of the crystals are either discrete or not so agglomerated as to be unmeasurable. The eye-piece micrometer is then turned through 360 degrees, and all discrete crystals encountered anywhere in the axis of the scale are measured. It follows, therefore, that some crystals will be measured along their long axes, some across, depending on the angle at which the revolving micrometer scale meets the main axis of the crystal. A large proportion of the crystals, however, were found to have been chipped and broken in the process of manufacture. A small agglomeration of crystals would be included in the count provided that the individual particles were distinct enough to be measured. When the field has been covered in this way, a fresh one is taken, and so on, until usually 300–50 particles have been measured. Entries were made in units of the micrometer scale (1 unit = $3{\cdot}4\mu$), any particle, for instance, either just over one unit or up to and including two units being entered as two units. Anything above 24 units formed a single group. All the measurements were carried out by the same observer (J. R.). When it was thought that a satisfactory degree of proficiency had been attained, the following experiment was made in order to test the consistency of the results.

Three different specimens of arsenic, one fine (No. 2) and two coarse (Nos. 16 and 7), were chosen by one of us (R. M. R.) and each specimen was measured three times by J. R., who received the samples in small, paper-covered tubes labelled merely *A*, *B*, *C*, . . . *I*. R. M. R. alone knew the key to the arsenics and the order in which they were given to J. R. The results of these counts are given in the first half of Table 2, where the frequency distributions are tabulated in micrometer units. These distributions are markedly skew and in order to test the consistency of the results

TABLE 2. *Results of parallel counts made on six different samples of arsenious oxide*

	Arsenic 2 Counts				*Arsenic 16 Counts*			*Arsenic 7 Counts*		
Units	*1*	*2*	*3*	*Units*	*1*	*2*	*3*	*1*	*2*	*3*
−1	38	38	43	−3	29	26	29	19	47	12
−2	41	30	36	−6	33	30	31	19	12	20
−3	14	16	23	−9	25	33	21	24	24	20
−4	6	7	4	−12	17	14	14	22	11	23
−5	1	5	..	−15	9	5	7	5	10	16
−6	..	1	2	−18	4	..	3	5	7	7
−7	..	..	..	−21	1	3	..	4	4	1
−8	..	1	..	−24	..	..	..	1	2	2
−9	..	1	..	> 24	..	1	..	5	2	2
−10	..	..	..							
−11	..	1	..							
Total	100	100	108		118	112	105	104	119	103
χ^2		10·57				4·12			38·25	
n		6				8			10	
P		0·20–0·10				0·90–0·80			< 0·01	

	Arsenic 3 Counts			*Arsenic 17 Counts*				*Arsenic 18 Counts*		
Units	*1*	*2*	*3*	*1*	*2*	*3*	*Units*	*1*	*2*	*3*
−1	52	55	53	36	49	43				
−2	30	27	38	40	33	34	−3	27	20	34
−3	14	17	8	17	20	14	−6	39	45	39
−4	2	4	4	8	8	7	−9	30	22	20
−5	4	4	..	2	2	7	−12	6	13	8
−6	..	1	..	2	2	2	−15	2	..	2
−7	..	..	..	..	..	..	−18	..	..	..
−8	..	1	..	1	1	..	−21	..	..	1
−9	1	..	..	..	..	..				
−10	..	..	1	..	..	..				
21–22	..	..	..	1	..	..				
> 24	1	1	..							
Total	104	110	104	107	115	107		104	100	104
χ^2		7·37			3·78				7·65	
n		6			6				6	
P		0·30–0·20			0·80–0·70				0·30–0·20	

1 micrometer unit = $3{\cdot}4\mu$

the smaller frequencies were grouped together as indicated by the brackets in the table, and the χ^2 test used. The first two values of χ^2 are satisfactory, while the third for arsenic No. 7 is excessive. Another series of three parallel counts on this last type of arsenic showed similar divergent results. In a further three specimens, two fine (Nos. 3 and 17) and one medium-coarse (No. 18), the values of χ^2 were 7·37, 3·78, and 7·65, there being 6 degrees of freedom in each case (for each sample three parallel counts were made, a fresh slide being prepared for each count, Table 2). Thus in this series of parallel counts made on six different samples of arsenic, five were satisfactory as judged by the χ^2 test, while in one sample χ^2 was twice excessive, indicating differences between counts larger than those which might be attributed to random sampling errors. The explanation for the divergences between counts observed in this last case may lie either in errors of technique (e.g. an unequal distribution of particles of different sizes on

TABLE 3. *Distributions of particle size of arsenious oxide*

Micrometer units	*Sample No.*							
	2	*23*	*20*	*31*	*5*	*26*	*21*	*7*
− 1	119	71	55	45	30	58	32	28
− 2	107	143	117	73	64	64	50	31
− 3	53	59	68	63	65	36	37	19
− 4	17	16	41	41	44	35	31	16
− 5	6	11	27	32	52	34	29	17
− 6	3	6	9	22	24	27	25	18
− 7	..	6	3	7	8	19	21	25
− 8	1	4	..	12	4	11	25	20
− 9	1	2	1	1	3	16	17	23
−10	..	3	..	8	3	5	14	25
−11	1	1	..	1	2	3	7	16
−12	..	1	..	..	3	2	10	15
−13	..	1	..	..	..	2	13	15
−14	..	..	..	..	2	..	5	7
−15	..	1	..	..	..	2	3	9
−16	..	..	..	..	1	..	2	7
−17	..	..	..	..	1	..	1	4
−18	..	1	..	..	1	..	4	8
−19	..	..	..	..	..	..	5	3
−20	..	1	..	..	1	..	3	4
−21	..	..	..	..	..	..	3	2
−22	..	..	..	..	..	..	2	1
−23	..	..	..	..	..	..	1	1
−24	..	..	..	..	..	..	..	3
24+	..	..	..	..	1	..	3	9
Total	308	327	321	305	309	314	343	326
Median	1·327	1·647	1·902	2·548	2·931	2·972	4·741	7·450
% < 3 units	90·6	83·5	74·8	59·3	51·5	50·3	34·7	23·9

1 unit = 3·4 μ.

the slide) or partly in a heterogeneity of the arsenic itself. The former source of error, however, seems the more probable.

There is no doubt that the technique could have been further improved, but so far as it was developed, the test appeared on the whole to give consistent results and afforded a quick method of assessing the coarseness or fineness of different samples of arsenic.

(ii) *Expressing the results*

In order to compare the relationship between particle size and the toxicity of a particular sample as measured by the L.D. 50 to white rats, it is necessary to express the results obtained by the microscopic method of measurement in some simple figure. The observed distributions of particle size are given in Table 3 for the eight arsenic samples for which we had any estimates of toxicity. With the exception of arsenic 23, all these were samples of refined arsenic. The arsenious oxide content was in most cases well above 95%, the lowest figure being 93% for arsenic 20. Arsenic 23 was a crude American sample and at the time these calculations were made its arsenious oxide content was unknown. It is evident that the distributions given in Table 3 are very far from being normal in form. It seems likely, however, that if the measurements had been done using some logarithmic scale, the resulting distributions would have been more nearly symmetrical. Thus, to take arsenics 2 and 5 as examples, curves were fitted to the observed data assuming that the logarithms of crystal diameter were distributed normally. The values of the geometric means were 1·210 and 2·767 units, and the standard deviations of the logarithmic measurements, $\sigma = 0{\cdot}7169$ and 0·6898 respectively. These estimates were made from the first and second moments of the observed distributions by the equations given by Malan (1937), disregarding in the case of arsenic 5 the single entry at 24+ units. The results of the fitting were as follows:

Micrometer units	*Arsenic 2*		*Arsenic 5*	
	Observed	*Expected*	*Observed*	*Expected*
− 1	119	121·5	30	21·7
− 2	107	112·1	64	76·9
− 3	53	42·8	65	70·4
− 4	17	16·6	44	48·4
− 5	6	7·6	52	31·3
− 6	3	3·5	24	19·9
− 7	..	3·9	8	13·0
− 8	1		4	8·4
− 9	1		3	5·6
−10	..		3	3·8
−11	1		2	2·6
11+	..		10	7·0
Total	308	308·0	309	309·0

In the case of arsenic 2 the fit is very good, while for arsenic 5 it would not be regarded as satisfactory if a χ^2 test were applied, principally owing to the large discrepancy between expected and observed in the 4–5 unit class. In broad outline, however, the theoretical distribution reproduces the main features of the observed. The chief point which we wish to make here is not that the logarithms of crystal diameter are in fact normally distributed, but that the distributions tend to be more normal and symmetrical in terms of logarithms than in absolute units. This being so, it was considered better to take the median particle size of the observed distributions, rather than the arithmetic mean, as a measure of the average diameter. Another measurement which we have used is the percentage of particles up to 3 units, or approximately 10μ diameter. The technique of measurement used here is not precise enough, in our opinion, to warrant much use being made of the many other ways of expressing particle size, some of which depend on the ratio between various moments of the distribution, and which are summarized by Dallavalle (1948).

(iii) *Toxicity*

For some of the arsenic samples (Nos. 2, 20, 31, 5, and 7) we have reasonably accurate estimates of the L.D. 50 for our stock of white rats. But, as already mentioned, this stock showed a change in its general level of susceptibility after the autumn of 1944. Tests done after this date with arsenic gave a significantly lower L.D. 50 than those done previously with the same samples. Before relating the toxicity of the samples to their particle size, it is evident that in the case of any tests done in 1944–5 some allowance should be made for this change in the animal stock (Table 4). Moreover, one of these arsenics (No. 7) was tested on male rats and in the early stages of our work some evidence was obtained that males were slightly more sensitive to arsenic than females. Some allowance, therefore, should also be made for the sex of the animals used in any particular test.

The log (Potency 1944–5/Potency 1942–3), estimated from the standard arsenic curves derived from tests made with sample No. 2 on female rats, was $0{\cdot}1134 \pm 0{\cdot}0215$; from the old and new tests with arsenic No. 5, also on females, it was $0{\cdot}1233 \pm 0{\cdot}0431$. It is clear that there is no significant difference between these estimates. The mean log ratio, obtained by weighting the individual estimates with the reciprocals of their variances, is 0·1154, and this figure was added to any estimate of the log L.D. 50 made on female rats during 1944–5 in order to make such tests comparable to those done in 1942–3. The log (Potency ♂/Potency ♀) was estimated to be $0{\cdot}0959 \pm 0{\cdot}0338$ from the tests with arsenic 2 done during the months of February, March, and April 1942. The observed log L.D. 50 of arsenic 7 was thus increased by 0·0959 in order to make it comparable to the tests using females. Since in the case of a very coarse arsenic such as sample No. 7 there was evidence that a number of deaths were delayed, the degree

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of mortality at the end of 8 days after administering the poison was taken as the end point, instead of at 5 days as in all the other arsenic tests.

TABLE 4. *Susceptibility to arsenious oxide before and after changes in the stock of rats*

Sample No.	*Series*	*Sex*	*log L.D. 50*	
			Observed	*Corrected*
2	Old	♀	2·0611±0·0171	..
2	New	♀	1·9517±0·0137	2·0671±0·0137
20	Old	♀	2·2344±0·0542	..
31	New	♀	2·0834±0·0138	2·1988±0·0138
5	Old	♀	2·2703±0·0302	..
5	New	♀	2·1436±0·0278	2·2590±0·0278
7	Old	♂	2·4428±0·0576	2·5387±0·0576

For two of the samples, Nos. 2 and 5, there was, after adjustment, a pair of estimates of the log L.D. 50. In each case a mean of the two values was taken by weighting the individual observations with the reciprocals of their variances, the variance of this weighted mean being given by the reciprocal of the sum of the weights. The resulting estimates of the log L.D. 50 were 2·0648±0·0107 for arsenic 2; and 2·2642±0·0205 for arsenic 5.

Some additional information was available for three further samples, Nos. 23, 26, and 21. The tests are on a very small scale for arsenic 23 and at only two dose levels each in the case of arsenics 26 and 21. The relative potency of these samples to the standard arsenic 2 was therefore estimated in each case. Thus arsenics 23 and 21, which were tested on males, were contrasted with the test of arsenic 2 on males done in February 1942, while arsenic 26, tested on females, was contrasted with standard arsenic curve for 1942–3. The results were:

Arsenic Sample No.	$log\left(\frac{\textit{Potency arsenic 2}}{\textit{Potency sample}}\right)$
23	0·3014±0·1048
26	0·2492±0·0502
21	0·3041±0·0402

These factors were then added to the log L.D. 50 of arsenic 2 (2·0611) in the old series of tests, and the resulting figures taken as estimates of the log L.D. 50 of these samples simply for the purposes of the calculations which are made in the next section. No adjustment was made for any differences in the arsenious oxide content between the eight samples.

(iv) *Effect of particle size*

When the corrected values of the log L.D. 50 were plotted against the logarithm of the median particle size, the relationship between the two variables was approximately linear. The one aberrant point was that for arsenic 23. It will be noticed, however, that the error attached to this estimate is relatively very great.

The eight observations which are available for determining the relationship between particle size and toxicity are set out in Table 5. Since the degree of error attached to each estimate differed greatly from sample to sample, the points were weighted with the reciprocals of their respective variances in fitting the regression line.

TABLE 5. *Corrected L.D. 50 of arsenious oxide in relation to particle size*

Sample No.	*Log median particle size* (x)	*Corrected log L.D. 50* (y)	*Weight* ($1/V$)
2	0·1229	2·0648	8748
23	0·2177	2·3625	91
20	0·2792	2·2344	340
31	0·4062	2·1988	5252
5	0·4670	2·2642	2390
26	0·4731	2·3104	397
21	0·6759	2·3652	619
7	0·8722	2·5387	301

If the observation on arsenic 23 with its relatively very small weight is neglected, the value of the regression coefficient $b = 0{\cdot}5576 \pm 0{\cdot}0495$; $t = 11{\cdot}3$, and for 5 degrees of freedom there is little doubt that this value is significantly different from zero ($P = <0{\cdot}001$). The inclusion of arsenic 23 makes little difference to this result: $b = 0{\cdot}5548 \pm 0{\cdot}0598$; $t = 9{\cdot}3$; $P = <0{\cdot}001$. The equations for the straight lines were:

$$\text{neglecting arsenic 23, } y = 1{\cdot}99362 + 0{\cdot}5576x$$
$$\text{including arsenic 23, } y = 1{\cdot}99568 + 0{\cdot}5548x$$

It is of interest to compare the estimates of the L.D. 50's which might be made from the regression line with those which were actually observed. It is assumed that the median particle size of each sample is known, as well as the conversion factors to allow for the time at which any test was done and the sex of the animals used. Table 6 shows the results calculated from the first of the above equations.

The agreement between the estimated and observed figures is reasonably good and in every case except one the calculated value lies well within the range of error attributed to the observed. The one exception is in the case of arsenic 23.

There is one additional item of information which supports the evidence given in this section for the relationship between particle size and toxicity. A sample of Canadian crude arsenic (No. 12) was found to be unmeasurable by the technique used here. Microscopically this powder appeared to be largely an aggregate of extremely fine particles and the crystals seen were very small. The general impression gained was that of an arsenic which was definitely finer than the standard No. 2. The L.D. 50 was estimated to be 112 mg. per kilo (female rats: old series) with a range of error ($P = 0{\cdot}95$) of 90–123 mg. Since this was a crude sample consisting of only 73% arsenious oxide, the true L.D. 50 was in the region of 82 mg. per kilo.

TABLE 6. *Observed L.D. 50 of arsenious oxide compared with that estimated from particle size*

Sample No.	*Series*	*Sex*	*Observed L.D. 50 (mg. per k.)*	*Range of error ($P = 0{\cdot}95$) (mg. per k.)*	*Estimated L.D. 50*
2	Old	♀	115	106–25	115
2	New	♀	89	84–96	88
20	Old	♀	172	76–209	141
31	New	♀	121	113–30	127
5	Old	♀	186	157–213	179
5	New	♀	139	121–61	137
7	Old	♂	277	156–362	242

Sample No.	$\frac{\text{Potency arsenic 2}}{\text{Potency sample}}$	*Range of error ($P = 0{\cdot}95$)*	*Estimated ratio*
23	2·002	1·228–6·739	1·128
26	1·775	1·411–2·271	1·568
21	2·014	1·724–2·620	2·034

This figure may be compared with that of 115 mg. for the finest refined arsenic which was measured in this series of experiments.

(v) *Discussion*

The analysis which has been made of these data shows a definite relationship between the median particle size of an arsenic sample and its toxicity as measured by the L.D. 50 to white rats. It would be difficult, however, to state categorically on this evidence alone that all toxicity tests of arsenic can be dispensed with, and that an estimate of the L.D. 50 can be made merely from the results of measuring the particle size of any particular sample. It is clear that a great deal more work is necessary, and that a properly designed series of experiments would be required in order to establish this relationship on a firm basis. Moreover, it does not necessarily follow that an arsenic which is classified in the laboratory as of a fine grade and of a high degree of toxicity, will be satisfactory as a poison in the field.

There is one further point: we have measured toxicity in terms of the dose which might be expected to kill on the average 50% of the test animals. But in relating toxicity to particle size there is no particular sanctity attached to the 50 per cent. point, apart from its convenience. We might equally well have taken the dose which would be expected to kill 90 or 95% of the test animals. In fact it might be argued that from the point of view of field work one or other of the latter measures of toxicity would be preferable to the L.D. 50. But the effect that this change of lethal dose would have on the results will depend on the slopes of the regression lines relating the probits to the logarithm of the dose. If the slope is also related to particle size, if for instance it becomes lower with an increasing degree of coarseness, the numerical results might be very different in taking the L.D. 90 instead of the L.D. 50. We have, however, obtained no clear-cut evidence that this is so. Thus the values of the slope (*b*) observed in the toxicity tests were as follows, the arsenic samples being given in order of increasing particle size:

Sample No.	*Slope b±S.E.*	
12	11·249±3·302	
2 (old)	6·941±1·025	7·662±0·771
2 (new)	8·599±1·168	
20	3·832±1·551	
31	14·824±3·701	
5 (old)	8·007±1·912	7·146±1·200
5 (new)	6·587±1·542	
7	4·256±1·622	

Testing for any differences between these eight estimates of the slope we have $\chi^2 = 15·34$, which for 7 degrees of freedom is on the high side, *P* lying between 0·05 and 0·02. There is thus some evidence that the slope was varying significantly. The suggestion in the table above that the slope is decreasing with increasing particle size is very slight, and we would prefer to interpret this significant value of χ^2 as a sign of a somewhat excessive variation from test to test in the value of this parameter. Assuming this latter hypothesis to be the sounder of the two, and given the distributions of the doses which were actually used in the tests and which in most cases lay equally above and below the 50 per cent. point, it seemed better to adopt the latter point as a measure of toxicity rather than to extrapolate the individual lines towards the higher dose levels. But clearly this question of the slope varying with particle size might be of some importance and it is a possibility which should be considered in the design of any future work.

As was mentioned briefly in an earlier section, no attempt was made to use any of the other measures in which the particle size of a sample can be expressed (e.g. Dallavalle, 1948). It is apparent from the types of distribution recorded for these arsenic samples that the median particle size, although a perfectly satisfactory measure so far as it goes, is in no way a comprehensive figure. Theoretically it would be quite possible to have two samples with very similar medians, but with a quite different spread of particle size around this point. In fact it can be seen from Table 3 that arsenic 23 is somewhat peculiar in this respect. It is a fine arsenic as judged by its median particle size, but compared with arsenics 2, 20, and 31 it has a much greater spread into the larger particle size classes. It is possible that the degree of toxicity might be affected in some way by such a feature of the distribution. One way of investigating this point was to consider whether, in addition to the median particle size, the log L.D. 50 might not also depend on the coefficient of variability round the median. That is to say, we took as a second independent variable the quantity $\sqrt{\{S(x-m)^2\}}/\sqrt{N}\,m$, where m is the median. However, the value of the partial regression coefficient on this latter variable was not significantly different from zero, and thus no evidence was obtained that this relative spread was affecting the L.D. 50's recorded in this short series of tests. But clearly the data are neither extensive enough, nor accurate enough, for a satisfactory test to be made of the more complicated types of relationship between the possible variables. We would prefer to regard the results so far obtained merely as a groundwork upon which the design of future experiments can be based.

(*d*) Added colouring matter

In Great Britain the Poisons (Colouring) Rules, 1936 (S.R. & O. 1936, No. 363), made under Section 23 of the Pharmacy and Poisons Act, 1933, state: 'it shall not be lawful to sell any poison included in the Schedule to these Rules and intended for use in agriculture or horticulture for the destruction of . . . vermin . . . unless there has been added to the poison a dye of a distinctive colour and soluble in water.' The Schedule contains only arsenical substances and includes arsenious oxide and arsenites. It will be noticed that these rules do not specify the colour or the amount of the dye to be used. It appears that, according to the letter of this law, it is not necessary to colour arsenic for rodent control in any but agricultural or horticultural premises.

In order to comply with the Rules, certain water-soluble dyestuffs are mixed with the arsenical substances by wholesalers or retailers. The dyes used in rodent control are, almost invariably, blue and seem, as a rule, to have been chosen because they are soluble and cheap; probably they have not been tested to see whether they are palatable to rodents. In some cases at least they have been chosen because they are known to be stable in the presence of arsenical acids and are fast to light.

Acknowledgement is made to the numerous firms and individuals who have given information concerning the dyestuffs used; certain of this information has been given in confidence so that no attempt will be made to show which manufacturers of arsenious oxide use which dyestuffs. The names of dyestuffs are often indicative of the manufacturing firm but it is essential to use these names because they apply to the actual grade or mark used in the tests given below. The following dyestuffs are known to be used. The number in the Colour Index 1923–4 is given.

Chlorazol blue 3 BS	(477)
Chlorazol sky blue FFS	(518)
Water blue 50118	(518)
Chlorazol blue B	(406)
Direct blue	(406)
Water blue 5009	(406)
Water blue BS	(406)
Water blue B 200	(406)
Lissamine green BS	(737)
Naphthol green	(5)
Green B 51040	(5)

Two further dyestuffs are known to be used, lamp black (1308) and ultramarine blue (1290), but these are insoluble and therefore do not comply with the Rules. Lamp black does however comply with the earlier Arsenic Act, 1851 (14 & 15 Vic. c. 13) which laid down that all arsenic sold, except in medical prescriptions, must contain soot in the proportion of one ounce to the pound or indigo in the proportion of half an ounce to a pound, i.e. 6% or 3% by weight. This act was repealed by the Pharmacy and Poisons Act, 1933 (23 & 24 Geo. V, c. 25). Ultramarine was, however, also sometimes used (Taylor, 1948). The following other dyestuffs have been recommended as suitable, but nothing is known of their use:

Durazol blue 2 GS	(1054)
Methylene blue 2 BS	(922)
Methylene violet 2 BS	(842)
Methylene violet RS	(842)
Brilliant YS crystals	(?364)
Magenta P 120	(677)

Three of these dyestuffs were tested in the field, chlorazol blue 3 BS (477), chlorazol sky blue FFS (518), and chlorazol blue B (406).

Chlorazol blue 3 BS (477) has been used commercially at a concentration of one pound per hundredweight of arsenious oxide, or 0·893%. At this concentration the dry powder is only faintly blue grey, but a fairly strong blue colour is given in wet baits. If arsenious oxide, so coloured, is mixed in a bait at 10% the dye would be at 0·0893%. Field tests using the dye at 0·5% in bread mash, roughly 6 times the concentration that would be used in practice, showed that the coloured food was accepted as readily as the plain. Similarly the presence of the dye in bread mash containing 5% arsenious oxide did not lower the expected take. Chlorazol sky blue FFS has been used commercially at a concentration of 5%; at this concentration the dry powder is bright blue, though in wet baits the resulting colour is no brighter than with chlorazol blue at 0·893%. The dye is at 0·5% if mixed in a bait with 10% arsenious oxide. Field tests using the dye at 1% and 0·5% in plain bread mash and with 5% arsenious oxide showed that the coloured bait was accepted as readily as the plain. Chlorazol blue BS (406) has been used commercially at a concentration of one ounce per hundredweight or 0·056%, or 0·0056% in a bait containing 10% arsenious oxide.

Field tests using this dye were less conclusive but gave no certain evidence that the dye was unpalatable.

These tests were carried out in 1941 and confirmed, in the case of chlorazol blue 3 BS by subsequent tests. This dye always proved palatable when used in bread mash to colour the faeces (Ch. 4); and with sample No. 26 arsenious oxide equally good results were obtained with and without the dye (Ch. 4, Table 51 (*a*)).

(*e*) Field tests

Details of field tests with arsenious oxide are given in Chapter 4. Fine samples were only available before the standard field testing technique was worked out; but at 10% they gave very high kills and even at 5% in some baits. Medium samples were very efficient at 15% in sugar-meal or at 10% on soaked wheat; but at 10% in sugar-meal they were less satisfactory. At 10% in the wet baits bread mash or rusk the evidence is less extensive, but several excellent results were obtained.

(*f*) Symptoms and post-mortem findings

White or wild rats poisoned in the laboratory show no symptoms which can be termed characteristic of arsenious oxide poisoning as opposed to any other metallic poison. Watery diarrhoea and a strong thirst are invariably shown, the animals are usually calm and they sit crouched in the cage until shortly before death. Death is preceded by a short period of convulsions. Time of death varies from 1½ hours to several days, being dependent on the nature of the sample as well as the size of dose. Onset of illness, time of death, and symptoms are discussed more thoroughly in Chapter 8. Superficial post-mortem findings are similar to those found in man and domestic animals and a detailed examination would probably show the same picture. Strong reddening of the stomach and some inflammation of the intestines are usually present. The usual death posture is on one side.

In colonies of wild rats which have been poisoned with arsenious oxide baits it is usual to find few corpses relative to the number found with zinc phosphide (Ch. 4). It is a common belief that rats poisoned with arsenious oxide do not give rise to smells of putrefaction. Whilst this supposition is false to the extent that a rat killed by arsenic in the laboratory will certainly putrify and smell, there are two possible foundations for this belief. Arsenical substances produce an embalming or mummifying effect on human corpses, and arsenic is an essential ingredient in embalming fluids. Taylor (1948) and Blyth and Blyth (1920) give examples of murdered persons remaining undecayed for long periods. In part this effect may be due to strong dehydration before death. Secondly, it is probable that many of the dead rats die deep in their burrows, from which the smell does not penetrate into rooms, rather than in such places as under floor boards or near the entrance holes to the burrows.

(*g*) Danger

Owing to its infamous history in the hands of poisoners, arsenious oxide has become a 'scare' substance, and the dangers of its use in rodent control have in consequence been exaggerated. It is however very toxic to man and to domestic animals and birds and proper precautions should always be taken both in making and in laying baits. In spite of its frequent occurrence in problems of human and animal toxicology, accurate information on toxicity is very scarce. This is largely due to the wide range of variation in the toxicity of different samples caused by differences in particle size and range and in the nature of the particles themselves, whether crystalline or vitreous; the solubility in the digestive tract varying with both these characters. This point was first stressed by Dr. H. H. Green (personal communication) when, from some experiments on sheep, he concluded that it was 'the rate of solution and not the extent of solubility which was the governing factor in the absorption'. It must be stressed that, in the cases given below, information is not usually given on the structure of the sample. Except for animals which cannot vomit, arsenious oxide possesses one safety factor in its emetic power: this is not strong, however, so that small but fatal doses may often be retained. An additional danger lies in its complete tastelessness in food. It is safe to handle and, unlike the soluble arsenites, is not absorbed through the unbroken skin, though it is slowly absorbed through skin lesions.

Actual cases of domestic animals and birds dying from eating rat bait containing arsenious oxide are not often published and tend to pass unnoticed. Such cases, particularly among poultry, are, however, fairly common. It is felt, as a result of our own observations, that cats are much less often killed by arsenious oxide than by zinc phosphide or barium carbonate. They have been observed to vomit after eating rats that they had caught during poisoning campaigns. Clough (1927) records a case of pigs becoming ill after eating rat bait containing 9% arsenious oxide and mentions cases in cattle, sheep, pigs, horses, dogs, and poultry.

Cases in man are also uncommon in Great Britain at least. The Registrar-General's Statistical Review (1919–20) and Report (1921–38) for England and Wales, records 2 accidental deaths and 5 suicides over this period of 20 years. In the same period 17 accidental, 36 suicidal, and 3 homicidal deaths were recorded with arsenious oxide of unstated origin, but sometimes, no doubt, connected with rat control. Arsenious oxide is not commonly used in proprietary rat poisons; with phosphorus, the most popular rat poison, there were 9 accidental and 50 suicidal deaths in these years.

Blyth and Blyth (1920), Taylor (1948), Glaister (1945), and other textbooks of forensic medicine give detailed accounts of toxicity to man. Fatal cases have occurred from as little as 130 mg. (2 grains), a dose corresponding to 2 mg. per kilo for a 140-pound man; but recovery has also taken place from 14·95 g. (230 grains), a dose corresponding to 460 mg. per kilo.

In the second case violent emesis removed most of the poison before it was absorbed.

General discussion on toxicity and symptoms for domestic animals are given by Nicholson (1945) and Schwartze (1922); both conclude that the scattered facts do not give a clear indication of the toxicity and that the range is wide. Nicholson gives figures of 10–45 g. per animal for the horse and 14–45 g. per animal for cattle. Fitch, Grimmett, and Wall (1939), however, found that doses of 1·367 g. on three consecutive days killed a heifer 4 days later. Nicholson gives 3–5 g. per animal for sheep, but Theiler (1912) found considerable variation; doses of 0·5–10 g. per sheep were found not fatal in individual cases. The toxicity was found to be increased if the animals were watered immediately after taking the poison, 2 out of 15 given 3 g. died 7 and 11 days later. St. John *et al.* (1940) killed sheep by doses of 0·4–0·5 g. per animal. Healy and Dimock (1921) administered unstated 'enormous' doses to pigs and conclude, in agreement with the popular belief to which they refer, that 'it would appear . . . that young hogs possess a marked tolerance' to arsenious oxide. Nicholson however gives a low fatal dose of 0·5–1 g. per pig.

For dogs Nicholson gives 0·6–1·5 g. per dog, though Doyon and Morel (1906) found that 18 g. for a dog weighing 16 kilo had no ill effect. Pocher (1911) also records the failure of 18 g. to kill a dog. De Busscher (1902) found a much lower figure of 15–20 mg. per kilo. Hausmann (1903) records a dog vomiting after 20 mg. per kilo and being very ill after 25 mg. per kilo. Very satisfactory figures are available for rabbits in Schwartze (1922) who administered doses of arsenious oxide of known particle size. He found that the 'certain lethal dose' for dissolved arsenious oxide was more than 15 but less than 20 mg. per kilo; for two fine samples it was 25 mg. per kilo (range of crystal size 0·0125–0·0025 mm.), and for a coarse sample 200 mg. per kilo (range 0·25–0·125 mm.)—a difference of 8 times between the coarse and fine. Schwartze carried out similar tests on chickens and found the corresponding figures to be 66·7, 75, and about 200 mg. per kilo. In this case there is a difference of only about 3 times, which may be accounted for by the grinding action of the gizzard. A similar figure of 0·1–0·3 g. per bird is given by Nicholson. Gallagher (1919) records the death of a chicken in 27 hours from 0·3 g. Game and wild birds are highly susceptible and Nicholson records wholesale deaths among pheasants. The U.S. Bureau of Biological Survey (1938) records the death of a quail (*Lophortyx* sp.) from 1·63 mg. of Schwartze's fine arsenious oxide. A study of the symptoms found in acute poisoning in small domestic mammals and birds has been made by Pistorius (1882).

(*h*) **Antidotes**

Antidotes for man and domestic animals follow the same principles. The stomach should be emptied as soon as possible by tube or failing this by emetic. Repeated gastric lavage should be carried out with large quantities of water, to which has been added freshly precipitated ferric hydroxide. The following antidote treatment is taken from Glaister (1945) which is based on the recommendations given in the Extra Pharmacopoeia (1943).

Four ounces of arsenic antidote (Antidotum Arsenum B.P.C.) should be left in the stomach and the dose repeated if necessary. To prepare this substance two solutions should be ready for use, namely, a strong solution of ferric chloride (288 minims) mixed with 2½ oz. of water, and light magnesium oxide (87½ grains) triturated to a smooth paste with water and diluted to 15 oz. Before being employed, 3¾ oz. of the suspension of magnesium oxide, after shaking, should be added to 400 minims of the ferric chloride solution. As a substitute 1 oz. tincture of ferric chloride in 4 oz. water with 1 oz. sodium bicarbonate may be given. The object of this treatment is to form insoluble ferric arsenite in the stomach. On account of possible dehydration, physiological saline should be given intravenously. Five per cent. glucose solution may also be given intravenously. Castor oil or magnesium sulphate may be given when vomiting subsides. Demulcent drinks may be given freely. For domestic animals Nicholson (1945) recommends a similar treatment and adds that dialysed iron, ½–1 oz. for dogs, 6–15 oz. for horses and cattle may also be used. Calcined magnesia or charcoal may be given to allay gastric irritation. Ravaut (1920), Taylor (1948), and Steyn (1939) all recommend intravenous and oral administration of 10% solution of sodium thiosulphate.

(*i*) **Summary**

1. Arsenious oxide, commonly called 'arsenic', has a long history as a rat poison, and at the present time is used extensively by government organizations, farmers, and gamekeepers. It is however rarely used in proprietary baits.

2. An analysis of numerous samples shows a wide range of particle size and impurities. The toxicity of any sample is shown to vary directly with the fineness of the particles contained in it, and L.D. 50's ranged from 88 mg. per kilo for fine samples to 242 mg. per kilo for very coarse samples.

3. White arsenical substances used for rodent control in Great Britain must have a dye added to them of distinctive colour and soluble in water. Lists of the dyes which are known to be used are given; certain of these, notably chlorazol blue 3 BS (477) have been tested in the field and have been found not to affect the efficiency of the poison.

4. Rats killed with arsenious oxide show no very characteristic symptoms; time of death varies with the amount and fineness of the poison from a few hours to several days. As a rule, few bodies are found after treatment with this poison.

5. Arsenious oxide is toxic to man, domestic animals, and birds but some safeguard is given, particularly to cats and dogs, because it causes vomiting.

Because of its infamous history, it is often considered a very dangerous poison, but this danger is no greater than that of any other poison of comparable toxicity.

4. RED SQUILL

(*a*) Introduction

Red squill, the powder or extract derived from the bulb of the red form of *Urginea maritima* (L.), has the most elusive and useful properties of all the poisons used in rodent control. At the present time it is the only plant product, used as such, rather than as a pure substance, that has an important place in rodent control. In common with all plant products which are used in pest control and in medicine it suffers from a great lack of constancy in its toxicity and consequent efficiency. In common only with the hellebores of rat-killing history it has a very strong reputation as a rodent specific. A poison which will kill noxious rodents and be harmless to all other animals is obviously an ideal weapon, but it can be shown, in fact, that red squill is very far from possessing such virtues. A poison which is most inconstant in its toxicity, and therefore cannot be recommended for use in standard bait formulae and methods, is an unreliable subject for research on rat control methods and principles. The fact that it has become universally accepted as the only specific is nevertheless a most powerful consideration, and extensive research upon it has been carried out. A good sample can give as good results as any of the other poisons, but it cannot be stressed too strongly that a sample of low toxicity is useless.

The history of this plant in pharmacy and later in rodent control, contains a number of interesting facets which reflect the properties attributed to it today. The native distribution of the squill plant corresponds roughly to that of the ancient world, and its medicinal properties were in consequence widely known to the civilizations of the Mediterranean basin. It is mentioned in the Ebers Papyrus (1500 B.C.) and Epimenides (650 B.C.), Theophrastus (*c.* 300 B.C.) and other doctors of ancient Greece knew its properties and used it extensively. The modern formulae of the British Pharmacopoeia are based on those of later Greece and Rome. Its toxic properties, however, were much less clearly known. The existence of two colour forms was known to Pliny and the red was recommended in preference to the white by the School of Salerno in the ninth century, a preference which survives in the French Pharmacopoeia today; but the fact that in general one was toxic and the other was not, escaped notice until late in the nineteenth century. Even in 1880 Bentley and Trimen wrote that there is, medically, no difference between the red and the white, though the white is sometimes preferred. Certain early textbooks of *Materia Medica* such as that of Crescenzi (*c.* 1304–6), first printed in 1486, note that the outer scales of the squill bulb are poisonous and must be thrown away before the rest is dried. Crescenzi is interesting because he was writing in Sicily and must have been familiar with the methods used for slicing and drying the bulbs. Parkinson (1629) states that the Spaniards considered it 'a most strong and present poison' and by the eighteenth century individual fatal case histories in man were on record.

Danzel (1935) has stated that the efficacy of squill as a rat poison has been known for a long time in the Mediterranean region. In the Middle Ages he states that fresh squames were chopped up and mixed with fat or meat to form baits, and he refers to a statement by Chomel (1767) who speaks of 'l'eau distillée de scille dans les appâts pour faire mourir les rats'. Chomel's dictionary was originally published in 1718 and an English translation by Bradley in 1725 contains the same statement. Other early statements of its use as a rat and mouse poison are given by Bergius (1778) 'muribus venenata', Chaumeton (1818), and Cazin (1868). Soubeiran (1857) gives a bait formula of one part of squill to four parts of smelly cheese or omelet. In spite of these it was relatively unknown as a rat poison until late in the nineteenth century and is not found amongst the poisons usually mentioned in English agricultural textbooks. Ableitner (1892) states that it was particularly popular in France, and the statement by Kohl (1895) that 'the fresh bulb has recently been much employed for killing rats, mice, etc.', probably indicates the beginning of its more wide popularity.

(*b*) Botany and industrial chemistry

(i) *Botany*

The red squill powder of commerce is the dried and ground squames of the red-bulbed form of the Mediterranean squill, *U. maritima* (L.) Baker, Liliaceae. The species is distributed throughout the sandy areas of countries fringing the Mediterranean, the trivial name meaning that it occurs in the Mediterranean rather than by the sea-shore; but it exists in a series of colour forms in which the bulb ranges from greenish-white through pink to dark red-brown. In general a white or pinkish form is dominant throughout the range of the species, except in the south-west Mediterranean seaboard; fairly dark red bulbs do however occur in most places. These pale colour forms appear to correspond to *U. littoralis* and *U. insularis* of Jordan and Fourreau (1866–70). The extreme red forms occur essentially in Algeria, Tunisia, and Morocco, and correspond to *U. numidica* and *U. sphaeroidea* of these authors. They have made a complete study of the distribution of the various intermediates and separated these four species, which varied in colour and in the shape of the bulb. These are now generally regarded as varieties or 'jordanons', a multiple allelomorphic series, grading into one another over the general range of the species. (This information was kindly supplied by Dr. W. B. Turrill.)

The bulb itself is very large when full grown, weighing up to 7 pounds;

the outer scales are dry and brittle and the inner more or less fleshy, varying according to the season. Like many liliaceous plants, the leaves and flowers are borne at different seasons of the year. The numerous dark green glossy leaves are put out at the end of the Mediterranean spring from January to March after the rains. They die off and fall away in May or June. The long flowering stalk with its white or pinkish flowers appears in September and continues until late October. The species is in general common enough in the regions in which it occurs to make cultivation unnecessary at the present rate of demand, but experimental cultivation has been tried in Algeria (Danzel, 1932, 1935) and in Italy (Parkinson, 1944; Green, 1944). Extensive experimental cultivation has been undertaken at several places in the southern states of U.S.A. (Hartzell, 1942; Green, 1944). Given suitable climate and soil it is apparently easily grown from seed or slices of bulb; full-sized bulbs grow from seed in about 7 years. The only pest recorded is a weevil *Brachycerus barbarus* ('Ionicus', 1936; Perris, 1874) whose larva, called by the Arabs 'donda', bores galleries in the bulb (Danzel, 1935).

The genus *Urginea* Steinheil contains some 40–50 species mostly from South Africa but extending also to India, and various of these species have been tested to discover their rat-killing potentialities. *U. Burkei* Baker, the Transvaal Slangkop, is a very dangerous stock poison and bulbs have been tested by Cushny (1915, 1916) who found them very toxic to rats and cats. (The bulbs were wrongly identified at the time as *Ornithoglossum glaucum* Salisb., see Watt and Breyer-Brandwijk (1932, p. 18).) Claremont (1922) found that one sample was fatal at 2,000 mg. per kilo but that two others were harmless at 1,400 and 3,000 mg. per kilo. The extremely mucilaginous nature of the bulb makes drying or extraction very difficult. George (1925) has studied this species as a potential substitute for *U. maritima*. Watt and Breyer-Brandwijk (1932) and Walt and Steyn (1939, 1941, 1943) have given information on this and several other species which appear fairly toxic. Thus one undetermined species of *Urginea* was fatal at a dose of 14 g. fresh bulb per kilo to a sheep of 28·2 kilo, while *U. rubella* Baker was fatal to a sheep of 44·1 kilo at 2·53 g. per kilo. If the fresh bulb contains about 75% water, as does that of *U. maritima*, powders derived from these species might have the same sort of toxicity as those derived from *U. maritima*. A small test carried out by the Bureau in 1941 with a powder derived from three bulbs of *U. Burkei* produced no symptoms in male white rats at doses up to 2·5 g. per kilo. Another test carried out by British Drug Houses Limited killed two male white rats at doses of 3·2 and 4 g. per kilo, the poison being given in water suspension by stomach tube; two others at 0·8 and 2 g. per kilo showed severe poisoning but recovered. Females receiving up to 5 g. per kilo in food were unaffected. In these species no suggestion has been made that the bulbs are relatively harmless to man and domestic animals. An Indian species *U. indica* Kunth. has been used as a substitute for *U. maritima* in pharmacy, and has been studied by Chopra and Mukherjee (1931); it is not known to be toxic.

(ii) *Preparation of powder*

A very detailed study of the collection and preparation of red squill in Algeria has been made and published in a series of papers by Danzel (1929–39) to which the reader is referred for full details. It is however essential to summarize his conclusions and those of other workers to obtain a proper understanding of the efficiency of red squill as a rat poison. The nature of the toxic content of red squill is dealt with more fully below but it may be said in general that red squill contains a heart glycoside similar in action to digitalis and a nerve glycoside which is a strong poison and is usually called the rat-toxic principle. This rat-toxic principle is absent from white squill and its content in the red varies partly with the depth of colour. The colouring matter itself is not however the poison. The amount of heart glycoside is apparently relatively constant throughout the year, but the amount of rat-toxic substance varies widely with the seasons. It is at its lowest when the bulbs are in leaf and have a very high water content and at its highest in the dry period just before flowering time, when the bulbs contain relatively less water.

There are three methods of preparing the red squill for market: a sun-drying primitive method, an oven-drying method, and one based on a stabilizing process followed by oven-drying. In all processes the bulbs should be harvested in the late summer, but, owing to the difficulty of finding the bulbs when they have no vegetation, they are picked when the leaves are just drying off or when the flower stalk is just visible. The bulbs are first chopped into small pieces about 2 in. long by a $\frac{1}{4}$ in. wide. These are the 'squames' of commerce and are, except in colour, identical with the white squill 'squames' which are marketed. In the sun-drying process these squames are now dried in the sun until brittle and ground to a coarse powder. The grinding may be done later and the squames exported as such. This process is still used to a considerable extent in Algeria and some markets have shown a preference for sun-dried red squill. It has, however, disadvantages; the drying may be uneven and slow; if the squames get rewetted by rain after partial drying, or by absorption from a damp atmosphere at a later period, enzyme action may start up with resultant destruction of some of the toxic principle. The third process was evolved to eliminate these dangers and is an attempt to produce a fairly standard powder which would remain stable for a long period. This is the so-called 'stablactivation' process described by Danzel and a monopoly of the Société Anonyme Levant Africaine Mediterrannée (S.A.L.A.M.) a subsidiary of Alais, Froges, et Camargue. This product, manufactured at Cherchell, was used extensively before the war in many European countries and in the United States. Almost all the work carried out by the Bureau has been done on various samples of stablactivated red squill. The process is based on two principles, (1) that plant products may be stabilized, that is to say enzyme action can be stopped, by subjecting them to methyl alcohol vapours, a method first described by Perrot and Goris (1909, 1935), and (2) that acetic acid is a very good 'activator' for squill, white or red.

After the bulbs are gathered the processing begins at once. The bulbs are first de-cored with a machine like an apple corer; this is to remove the white fleshy centre squames which are less toxic. The remainder is then sliced up into the 'squames' of commerce, drained to remove any water liberated by the cutting and dried in a drying tunnel at 80–100° C. for a short time. The squames are then charged into the 'stablactivateur', the liquid being a mixture of methyl alcohol and acetic acid, and left there for about ¼ hour. The end point of the reaction is when the enzymes have been destroyed. This is tested for with a tincture of guaiacum with hydrogen peroxide: a blue colour is produced in the presence of peroxidase. The squames are then removed to a drying tunnel where they remain at a temperature of about 55–70° C. for about 20 hours. The dried squames weigh about 30% of the weight of the ordinary bulb; their moisture content is about 8%. The squames are finally ground either to a coarse powder, called M.I., of which about 60% is retained by a 60-mesh sieve, or to a very fine powder, called M.O. or 'fleurs de scille', corresponding to about 180 sieve. The powder is exported in wooden barrels and must be kept as dry as possible or it will cake. The final product is brownish-red with a peculiar musty smell and bitter taste.

(iii) *Chemical constituents*

While it is not intended to deal with the chemical constituents of red squill in detail, nevertheless it is important to realize that it contains a number of different chemical substances, the toxic principle or principles representing only a small fraction of the whole. The presence of other substances might be of importance in the relative harmlessness of red squill to other animals, and while it is known that the extracts prepared by the methods described in this chapter are relatively harmless to chickens and cats, it is not known what effect may be produced by the rat-toxic principle itself or any partly purified extracts.

The early work on isolation of various substances has been summarized by George (1925). From Boerhave (1741, in George), through the always quoted work of Merck (1879) and of Kopaczewski (1914), a large number of different substances, some responsible for the cardiac action common to red and white squill, some for other medicinal effects, and some for the rat-killing action of red squill alone, have been described, but the majority of these are impure amorphous powders. Names, such as scillin, scillinin, scillitin, scillitoxin, scillidiuretin, xanthoscillide, scillipicrine, scillimarin, are scattered through the extensive literature and are names for effects rather than for chemically pure substances. The recent work of Stoll and his co-workers in Basle (1940–2 and G.B. Patent No. 535, 5721941) and of Picard (1936) has done a great deal to clear away this excess of names. The cardiac action of red or white squill is due to two or more glycosides; of these scillarene A and scillarene B have been isolated in crystalline form. Scillarene A, the principle glycoside of white squill, has the empirical formula $C_{36}H_{52}O_{13}$; it has a toxicity corresponding to at least 50 g. of the bulb and therefore plays no part in the toxicity to rats; further, it is present in the bulb all the year round. Scillarene B, an amorphous powder, is probably a mixture, and the red colouring matter is also a glycoside which is apparently non-toxic to rats.

The latest work of Stoll and Renz (1942 *a*) describes a crystalline glycoside with a very high toxicity to rats; this they call scilliroside, with an empirical formula of $C_{32}H_{46}O_{12}$ and closely related to the scillarene group. It has a L.D. 50 of 0·70 mg. per kilo for male rats and 0·43 mg. per kilo for female rats, the same differential toxicity between the sexes being found with the pure substance as with the powder. It is stated that a maximum of 370 mg. of scilliroside can be obtained from one kilo of fresh bulb harvested at the correct time of year: this corresponds, in the case of male rats, to an L.D. 50 of about 1,900 mg. per kilo rat of fresh bulb or 380 mg. per kilo rat of powder, if the bulb loses 80% of its water on drying. The last figure is very close to what would be expected of a good powder sample. Picard (1936), whose work has been summarized by Bijlsma (1936), has produced a glycoside with a probable empirical formula of $C_{15}H_{20}O_6$ which has apparently an even higher toxicity of about 0·2 mg. per kilo rat; but the method of bioassay is not given. It is possible, as Stoll and Renz suggest, that the rather violent chemical processes by which this substance was derived may have broken down the molecules of the true toxic agent to give smaller molecules with higher toxicity; it is also possible that the toxic agent in the plant is not present in so definite a form and that each process gives a different but closely allied glycoside. Other authors such as Leblanc and Lee (1939), Winton (1927 *b*), and Gross (1940) have used refining processes on their initial extracts, obtaining final products which killed at about 10, 30, and 5 mg. per kilo respectively; their methods were similar in principle to that of Stoll and Renz.

(iv) *Methods of extraction*

Red squill powder may be used in baits as such or it may be subjected to an extraction process and the resultant extract used in its place. The reasons that make it necessary to extract certain samples are discussed later, but the actual methods which have been used must be summarized here.

Provided that a solvent is used in which the toxic principle is fairly soluble, the method of extraction appears to be unimportant except when the object is a commercial one and the aim is 100% extraction of the toxic principle at lowest cost. The methods described in the extensive literature are not strictly comparable because of the many variables and frequent absence of efficient toxicity tests on the powder and resulting extract. It is considered most convenient therefore to discuss these methods first according to the solvent used, rather than by method or by efficiency of results.

Several authors have made water extracts, though it is stated by Stoll and Renz (1942 *a*) that the toxic principle is only slightly soluble in water; larger quantities of solvent and longer maceration may therefore be necessary. It is probable that some of the toxic principle will be lost by hydrolysis and the extracts are never as toxic as those produced with organic solvents. Claremont (1922) produced water extracts by macerating 125 g. of chopped fresh bulb for 24 hours cold or 4 hours boiling and 20 hours cold, with or without a trace of hydrochloric acid, with 400 c.c. of water. The extract strained and pressed and made up to 500 c.c. was fatal to rats at about 15–19 c.c. per kilo; a useless toxicity if the extract is to be made up with bait. Winton (1927 *b*), by extraction at various temperatures from cold to boiling, produced an extract which would kill white rats at 400 mg. per rat, the toxicity of the original powder being about 1,600 mg. per kilo. Munch, Silver, and Horn (1929) extracted Sardinian, oven-dried powder of 'M.L.D.' about 500 mg. per kilo with water. They used percolation, exhaustion, and soxhlet methods but all their extracts were of extremely low toxicity. Buck and Fellers (1935) also tried water extraction in a soxhlet but the extract was non-toxic at 2,000 mg. per kilo. Mann (1937) describes a percolation method of preparing water extracts and considers them reliable, though he gives no figures for toxicity; a similar method is described in Anon. (1936).

Methyl and ethyl alcohols have been used in a number of experimental extractions. The idea that methyl alcohol is more satisfactory than ethyl is largely derived from Buck and Fellers, who came to this conclusion in a comprehensive series of tests of both soxhlet and percolation methods. It is also stated, though without evidence, by Mann. In methyl alcohol extractions, with time of extraction varying from 1 to 16 hours, they found that with 4, 8, and 16 hours' extraction the extract would kill four out of four rats at 500 mg. per kilo powder equivalent. They do not state the amount of extract obtained, so that the absolute toxicity cannot be worked out. Methyl alcohol extracts were not effective when prepared cold by shaking and stirring for times up to 6 hours. A number of extracts at varying temperatures and for varying times were prepared by percolation, and the best of these killed four out of four rats at 500 mg. per kilo powder equivalent. Perhaps the most surprising point about this series of extractions is the extremely low yield of extract produced. One extraction, in which the powder was soaked in methyl alcohol in a ratio of 3:1 for 3 days at a temperature of 25° C., gave a yield weighing 0·9% of the original powder. The best yield was one of 14·6% after only 4 hours at 60° C., the ratio of solvent to powder being 6:1. From the figures given for percentage extraction, the absolute toxicity of the extract can be worked out. In the case of the 0·9% yield it appears that three out of four rats were killed at a dose of 4·5 mg. extract per kilo; this figure is extremely low and represents an extract more than 100 times as toxic as the powder from which it was derived.

Buck and Fellers also made extracts with ethyl alcohol by soxhlet and by percolation. In soxhlet extractions the toxicity of 8-hour extracts was apparently as high as those with methyl alcohol; 4 hours was not tried but 2 hours appeared to be slightly less efficient. Three percolation tests were decidedly less efficient than those using methyl alcohol. Munch, Silver, and Horn extracted for 162 hours in a soxhlet with ethyl alcohol and obtained an extract of 'M.L.D.' about 200 mg. per kilo. from a powder of 'M.L.D.' 500 mg. per kilo, the yield being 14·0% by weight of the powder. Crabtree, Ward, and Garlough (1942) used ethyl alcohol in their maceration rotation method to produce an extract for their 'Fortification' process. After a total maceration time of 3 days, they produced an extract of 'M.L.D.' 125 mg. per kilo, that of the original powder being about 2,500 mg. per kilo. Stoll and Renz state that the toxic principle is easily soluble in methyl and ethyl alcohols.

Cellosolve (ethylene glycol mono-ethyl ether) and glycerol are mentioned by Buck and Fellers as other suitable solvents, although extracts produced with the former were not so toxic as those produced with alcohols. Stoll and Renz state that the toxic principle is soluble in ethylene glycol, dioxan, and glacial acetic acid. Buck and Fellers found that acetone, diethyl ether, and ethylene dichloride produced either very little extract or a non-toxic one. Munch, Silver, and Horn found that acetone and chloroform were unsuitable; Winton, however, used acetone in purifying his extract, dissolving the extract in 9 times its own volume, and Stoll and Renz state that the toxic principle is fairly soluble in acetone.

It is clear that, provided a suitable solvent is used, almost all methods will produce a substance which is, to some extent, toxic to rats. The methods fall into two more or less sharply defined classes: those that are attempting to produce an extract which will kill rats in the field and have therefore a commercial application, and those which are, at a research level only, attempting to discover the chemical nature of the rat-toxic principle. The objects of extraction on a commercial scale are threefold: to make use of red squill powders whose toxicity is too low for them to be used in powder form; to produce liquid baits, because they have some advantage, supposed or real, over powders; and to produce a poison which is supposed not to cause shyness in survivors of a first treatment. The first object is certainly the most important and the one which has necessitated the researches on extraction described in this chapter. For various reasons samples of squill powder may be of such low toxicity that they cannot be used in powder form in baits; nevertheless, as they are often in short supply and expensive, they cannot be discarded altogether. By extraction a smaller weight with higher toxicity can be produced. Large-scale processes have not been described in detail in the literature but the method used by Crabtree, Ward, and Garlough (1942) has been used on a very large scale in the U.S.A. as a basis for their fortification process.

O'Connor, Buck, and Fellers (1935) have stated that extract baits, the extract being made with ethyl alcohol or glycerol, are preferred to powder baits, a three to one preference being indicated. However, this does not mean that equally satisfactory control cannot be obtained with powders. The statement that extract does not cause shyness in survivors of a first treatment is also due to these authors. 'The interesting observation was made repeatedly that rats which recovered from the effects of sublethal doses of red squill powder could rarely be induced to eat squill baits again, even after a lapse of several months. These same animals readily consumed baits containing alcohol or glycerol extracts of red squill. Similarly, extract baits were eaten repeatedly in sublethal doses.' It is not stated on what rats these tests were carried out but it may be inferred that they were white. Evidence from the Bureau's toxicity tests confirms these observations on white rats. Tests carried out on rats which had already received extract baits within the previous 14 days are summarized in Table 13. In no case was the bait refused. However, there is evidence in our own work that wild brown rats (*Rattus norvegicus*) will not take the extract more than once, at least in the same bait base (Ch. 4).

Soxhlet and percolation extractions seem to be equally suitable when the experiments are on a small scale e.g. to produce the toxic principle, while maceration and simple stirring have not usually given such good results. On a commercial scale, soxhlet extraction is not practicable, and percolation seems to be the most suitable method where large amounts are concerned, though a large-scale maceration process has been used extensively in the U.S.A. From small-scale extractions, particularly those of Buck and Fellers (1935), it appears that at least some heat (about 60° C.) is necessary to produce a satisfactory extract, but this need is probably overcome by increasing the extraction time. Soxhlet apparatus, of course, necessitates the extract remaining at the boiling-point of the solvent throughout the extraction. The evidence as to whether heat affects the stability of the toxic principle is conflicting, but it is probable that continuous heat in water extractions will cause hydrolysis, while little or no effect is produced in alcohols. Stoll (1927) finds that the pure toxic substance melts at 160–170° C. but that it does not decompose until about 200° C.

The time of extraction, in the small-scale tests, seldom exceeds 24 hours and is more often 2, 4, or 8 hours. It is probable that the shortness of time of extraction considerably affects the amount of extract produced and perhaps also its toxicity. In the Bureau series of tests detailed below, 8-hour extraction produced an extract representing 15% by weight of the powder from which it was derived, while 24 hours' extraction produced

37%. The amount of solvent used, relative to the amount of powder, varies much in the individual tests. In soxhlet extractions the amount does not matter provided that there is always a little more than the amount required to work the siphon, but in percolation and maceration tests the solvent should always be considerably in excess of the powder; this will be particularly important with solvents such as water in which the toxic principle is not very soluble. The amount of extract produced therefore varies with time, temperature, and nature and amount of solvent; in extreme cases of water maceration, Munch, Silver, and Horn (1929) found that 80% of the original weight of powder was extracted. It must be remembered, however, that the object of the process is to reduce the bulk whilst retaining its toxicity, so that the ideal extract is one which contains all the toxic substance but as little other matter as possible; at the same time the cost of the process must be kept low. The normal coarse stablactivated powder (M.I.) is fairly suitable for extraction by soxhlet or by percolation, being coarse enough to allow full penetration by the solvent. Fine powders (M.O.) pack very easily and in consequence do not allow thorough penetration. Even the coarse powders absorb a large amount of the solvent and draining is very slow.

(v) *Experimental extractions*

A series of small-scale extractions has been carried out by the Bureau using a soxhlet apparatus, with methyl alcohol as a solvent. The red squill powder used throughout was stablactivated M.I. from S.A.L.A.M., being a part of sample S 1; it had a toxicity of 443 mg. per kilo for male white rats. The percolator was copper lined with zinc and had a capacity for 735 g. of powder, about 900 c.c. One litre of I.C.I. methanol AR was used in each extraction. A fairly constant rate of boiling was obtained by keeping a constant water level in the jacket and careful gas regulation so that the number of cycles was about one per hour. The temperature of extraction i.e. that of the upper chamber was about 40° C., that of the lower chamber was, of course, the boiling-point of methyl alcohol (66° C.). Five early tests were carried out in a small glass soxhlet having a capacity for only 45 g. of powder; this was abandoned, however, because the amount of extract produced was so small that it was impossible to carry out field tests with it. After extraction the methyl alcohol was distilled off, the pressure being reduced to slightly less than one atmosphere when the majority of the alcohol had come over, to prevent bumping and a rise in temperature. The details of these tests are summarized in Table 7.

The final product is a dark red resinous liquid. The w/w total solids (i.e. weight of the solid compared with total original weight) was not worked out for this extract, but, by comparison with the commercial extracts where this figure is known, it is probable that the liquid content was between 5–10% w/w. The extracts were made up for use by adding water until the volume of the whole in c.c. was equal to the weight in grammes of the powder from which it was derived. Some parts of the extract are water soluble so that the water added was not equal in weight to the difference between the weights of the powder and extract. They were used in this form in the field tests and the percentage of extract used was worked out in terms of powder equivalent.

TABLE 7. *Details of laboratory extractions of red squill*

Extract sample E No.	*Soxhlet capacity*	*Hrs. time of extraction*	*Amount of powder (g.)*	*Amount of solvent (c.c.)*	*Amount of extract (g.)*	*Extract % of powder by wt.*
1	Small	6	45	200	10	22
2	,,	6	45	200	10	22
3	,,	12	45	200	. .	. .
4	,,	6	45	200	10	22
5	,,	6	45	200	. .	. .
6	Large	8	350	1,000	50	14
8+9	,,	24	735	1,000	250	34*
10	,,	24	735	1,000	295	40
12	,,	24	540	1,000	205	37
13	,,	24	735	1,000	280	38
14	,,	8	735	1,000	130	18 †
14	,,	8	735	1,000	110	15

* Sample divided in two halves for removal of MeOH.
† Two extracts added together.

A large-scale extraction process was worked out in conjunction with Messrs. Stafford Allen Ltd. for amounts up to one ton at a charge, and we are grateful to them for their help and permission to describe the method. The sample used throughout these tests was B.A.P. sample S 4 which was an 'M.I.' coarse stablactivated powder with a L.D. 50 of 805 mg. per kilo to male white rats. The plant consists of (*a*) a solid copper vertical cylinder of approximate diameter 4 ft. and height 12 ft., fitted with a perforated metal plate covered by a filter cloth, fixed about one foot from the dished bottom of the cylinder and a closed steam coil in the space between the perforated plate and the dished bottom. This vessel will hold one ton of red squill powder comfortably; (*b*) a horizontal storage vessel in copper of about 250 gal. capacity below the cylinder (*a*); (*c*) a still of 50–60 gal. capacity in copper; (*d*) another still of 30–40 gal. capacity in stainless steel.

About 2 cwt. of red squill powder is charged into the extraction vessel (*a*) and sufficient methanol (100%) added to wet it thoroughly. Another 2 cwt. of powder is then added and again sufficient methanol to wet it thoroughly. This is continued until the whole of the powder has been added. Finally, methanol is added until there is a depth of 8–10 in. above the level of the powder. About 400 gal. of methanol are required per ton of powder.

The vessel is then closed and left for a period, after which liquor is drawn off through a cock below the perforated plate into the storage vessel. When about 250 gal. of liquor have been drawn off, which requires about a day and a half, the draw-off cock is closed and 250 gal. of fresh methanol are added to the extraction vessel.

The draining period is very long owing to the fineness of the powder. It is not possible to put pressure on the extraction vessel to decrease draining time because clogging of the filter would result. Extraction from squames, or shavings about $\frac{1}{8} \times 1$ in., as is the usual practice for white squill, might eliminate this difficulty. The only likely disadvantage in using squames lies in the fact that a hard outer skin may be formed which might retard penetration by the solvent.

TABLE 8. *Details of cold bulk extractions of red squill*

Percolate No.	*Maceration time, days*	*Vol. in gals. 60% total solids*	*% total extract*	*Total solids % w/w after removal of* MeOH
1	7	32·25	35·80	86·0
2	3	30·00	33·30	81·1
3	3	10·50	11·60	81·8
4	7	8·00	8·8	76·2
5	3	3·75	4·1	88·6
6	3	2·50	2·7	92·1
7	3	1·87	2·07	91·3
8	3	1·15	1·25	85·2

TABLE 9. *Details of hot bulk extractions of red squill*

Percolate No.	*Maceration time, days*	*Vol. in gals. 60% total solids*	*% total extract*	*Total solids % w/w after removal of* MeOH
1	7	39·25	42·38	75·1
2	3	21·32	24·27	72·6
3	3	18·68	18·76	77·8
4	7	3·45	3·53	77·6
5	3	2·75	2·61	75·5
6	3	3·12	3·60	81·8
7	3	2·86	3·70	76·9
8	3	1·50	1·07	72·1

The liquor is transferred from the storage tank to the copper still and methanol removed under reduced pressure (vacuum approx. 60 cm.; temperature approx. 40° C.) until the concentrate contains about 32% total solids. This concentrate is moved into the stainless steel still and further methanol taken off until a total solid content of 60% is reached. The temperature in both stills is not allowed to rise above 60° C. The concentrate of thin treacly consistency is now transferred either to large stoneware jars or to a stainless steel cylindrical open-ended tank. Alternatively the remaining methanol may be removed at once in still (*d*), leaving a thick treacle.

Two test extractions were carried out each on one ton samples, the first cold and the second at about 40° C. Each batch was given eight percolations. Figures for the amount of extract derived from each percolation are given in Tables 8 and 9. Assuming that the toxic principle comes over uniformly in all percolates it is clearly uneconomic to take more than four percolations in each extraction, as only 10·12% and 10·98% of the whole is added in percolations 5–8.

Diluted extracts are very liable to grow moulds on the surface and some observations have been made on the point of dilution at which they do so. Bulk extracts with 50% of water added remained free from surface moulds without the addition of mouldicides for 4 weeks, when the test was discontinued. Laboratory extracts containing about 85% water, in closed containers have remained free from moulds and without apparent fermentation for 2 years, while those containing 97% water grew moulds freely in a few days.

(*c*) **Toxicity of powders**

(i) *Range of toxicities*

The problem of the toxicity of any given sample of red squill is a very complex one, and a large number of variables must be considered before any answer can be given as to whether any sample is likely to be of use in the field. It is not possible to lay down hard and fast rules that powders below a certain toxicity are unsuitable without extraction or further processing, but it is essential that all samples should be tested for toxicity by some rigidly standard bioassay procedure. Toxicities so found can then be compared against a standard of known efficiency in the field. It has been suggested that it might be possible to estimate toxicity by some colorimetric method sensitive to the rat-toxic principle, thus obviating the need for expensive and protracted bioassay. Stoll and Renz (1942 *a*) have given the reactions of scilliroside to various colour tests, the only positive test being a Liebermann with acetic anhydride-sulphuric acid mixture, in which the change is from violet through green to blue-green. Recent unpublished work in America has shown that it is not yet possible to estimate scilliroside content by this or many other methods.

Some of the variables caused by differing sensitivities of the stocks of rats have been discussed in § 2. Different stocks certainly show widely different susceptibilities. Caged brown rats are said to be more sensitive than white rats (Claremont, 1922; Munch, Silver, and Horn 1929; Munch, 1937; Bahr, 1941). O'Connor, Buck, and Fellers (1935) indicate that white rats in higher weight groups are more resistant per kilo body weight than those in lower weight groups. Evidence from our own tests with sample S 1 indicate that change of animal house routine may have affected the susceptibility of the stock, though the evidence is not so conclusive as with arsenious oxide. Ward, Spencer, Crabtree, and Garlough (1940) give tests which indicate that toxicity to a given rat stock may vary widely at different altitudes. Danzel (1935) has suggested that toxicity may increase with increasing latitude but the evidence is very slender.

The different sensitivity of the two sexes has long been recognized and is well authenticated, though Bahr (1941) states that he was unable to find any difference. Claremont (1922), Winton (1927 *a*), Leblanc and Lee (1939), Crabtree, Ward, and Welch (1939), and Gross (1940) have all found that female white rats are more susceptible than males. Crabtree, Ward, and Welch also showed that spayed females remained as sensitive as normal females, whilst castrated males were more susceptible than entire males. Stoll and Renz (1942 *a*), working with the pure scilliroside, were also able to show this difference, the L.D. 50 for males being 0·70 mg. per kilo, that for females 0·43 mg. per kilo. This difference of about a half corresponds closely to that usually found with the powder.

Apart from the variation of sensitivity of the rat stock, the toxicity of a sample may vary with five main factors, the source of the original bulb, the colour form or other variation within samples from one broad locality, the season of harvesting, the method of processing, and the age of the powder.

(ii) *Bioassays*

Samples have been received and tested by the Bureau from Algeria, Portugal, Italy, Malta, Cyprus, Egypt, and Palestine; and it will be pertinent to discuss their toxicities according to their source in conjunction with the published findings of various authors on samples from these and other countries. Algerian material may be either the stablactivated product of the S.A.L.A.M. factory at Cherchell, or sun-dried; and it has been, according to Danzel (1935), used extensively throughout Europe and in the U.S.A. Some statements on toxicity without detail are given by Danzel (1932, 1934, 1935) and it is probable that the toxicity tests given in some of the other papers also refer to powders of this origin. S.A.L.A.M. powders may be coarse (M.I.) or fine (M.O.) and are brownish-red in colour, the fine powders being paler than the coarse.

We are indebted to Dr. J. Raventos of Imperial Chemical (Pharmaceuticals) Ltd. in Manchester for carrying out some of the toxicity tests.

Seven separate war-time shipments of S.A.L.A.M. powder have been tested by the Bureau: the essential details of these tests are given in Table 10 and it will be seen that the L.D. 50 to a constant stock of white rats ranged from 357 to 883 mg. per kilo. Another sample (S 2 *b*) produced no deaths below 1,250 mg. per kilo.

Sample S o (original S.A.L.A.M.). Received and tested June 1940 and later. Coarse powder. This sample was tested at four dose levels on young male rats (T.002 *a*) and at another four doses on old male rats (T.002 *b*), the L.D. 50's being 541 mg. per kilo and 381 mg. per kilo respectively. However, since no significant difference was found between either the position or the slopes of the individual regression lines for young and old, the results were pooled and a single line was fitted to the combined data. The L.D. 50 estimated in this way was 450 (275–601) mg. per kilo. The claim of the manufacturers, that this powder should kill 50% of male white rats at doses between 400–500 mg. per kilo, thus appears to be fully borne out by this sample.

TABLE 10. *Summary of toxicity tests on Algerian and Sicilian red squill powders*

Squill sample	*Laboratory*	*Test number*	*Slope (b)*	*log L.D. 50*	*L.D. 50 (mg. per kilo)*	*Range (P = 0·95) (mg. per kilo)*
S 0	Oxford	T. 002*a* and *b*	3·179 ± 0·912	2·6530 ± 0·0694	450	275–601
S 1	,,	T. 20, 21	6·354 ± 1·430	2·6218 ± 0·0385	419	336–496
S 1	,,	T. 43	4·400 ± 1·202	2·6882 ± 0·0446	488	392–632
S 1	,,	T. 61, 62	6·655 ± 1·943	2·5746 ± 0·0322	384	311–444
S 1	,,	T. 20, 21, 41, 43, 49	4·530 ± 0·796	2·6463 ± 0·0293	443	380–504
S 1	,,	T. 58, 61, 62, 63, 64, 65, 73	4·301 ± 1·023	2·5526 ± 0·0341	357	285–418
S 1	,,	Ditto, excluding T. 58	4·342 ± 0·794	2·5971 ± 0·0239	395	354–446
S 2 M.O.	Manchester	M.R. 5, 7	7·431 ± 1·879	2·7289 ± 0·0266	536	464–612
S 4	Oxford	T. 39, 40, 48	2·481 ± 1·164	2·9060 ± 0·0780	805	256–1,619
S 5 Coarse	Manchester	M.R. 12	8·615 ± 1·848	2·8784 ± 0·0258	756	664–859
S 6	Oxford	T. 57, 66	2·225 ± 1·118	2·9340 ± 0·0701	857	323–∞
Sicilian	,,	T. 003	4·653 ± 1·275	2·9460 ± 0·0439	883	675–1,085

Sample S 1. Received February 1943. Coarse powder. The original tests of this sample (T. 20, 21) made in February 1943 gave a L.D. 50 of 419 (336–496) mg. per kilo. A second full test (T. 43) of this sample carried out some 11 months later in January 1944 showed no significant change in toxicity, the L.D. 50 being 488 (392–632) mg. per kilo. This was confirmed by smaller tests (T. 41, 49) done after 10 and 18 months. A third series of tests was carried out during December 1944 (T. 61, 62) and the L.D. 50 was found to be 384 (311–444) mg. per kilo. At this time, however, there was some question as to whether or not the rat stock had become more susceptible to the various poisons tested. The evidence, which has already been discussed, was indefinite in the case of squill, but very definite in the case of arsenious oxide. It is possible therefore that this estimate of 384 mg. per kilo may have been low as compared with those found in the old series of tests made before the change in the rat stock took place. Even if we were to assume that the animals had become 30% more susceptible to squill, the degree to which they had changed towards arsenious oxide, the adjusted L.D. 50 of this last test would only be 499 mg. per kilo instead of 384 mg. Further tests (T. 64, 65, 73) made later still, the last being in March 1945, showed again that there had been no very appreciable diminution in the toxicity of this sample. Thus the general run of this evidence indicates that when kept in sealed tins the loss, if any, in toxicity was only slight over a period of some 25 months. By combining together all the results of the tests made on this sample between February 1943 and July 1944, the L.D. 50 was estimated to be 443 (380–504) mg. per kilo; and from the new series of tests made between December 1944 and March 1945, it was 357 (285–418) mg. per kilo. The difference between these results cannot be regarded as significant. It is of course possible that an increase in the sensitivity of the animal stock was to some extent balanced by a slow deterioration of the squill powder over a period of some two years.

Sample S 2. Received June 1943. This shipment consisted of both coarse and fine powders. From the results of two tests carried out at Manchester on the coarse grade it was found that L.D. 50 was greater than 1,250 mg. per kilo and thus this powder appears to have been relatively inert. This was also confirmed by a small test done at Oxford in which no deaths occurred up to a dose level of 1,200 mg. per kilo. On the other hand, the fine, also tested at Manchester (M.R. 5, M.R. 7), was of satisfactory toxicity, the L.D. 50 being 536 (464–612) mg. per kilo. From some comparative tests which were made at the same time on sample S 1, it appeared that the rat stock used at Manchester was somewhat more resistant to red squill than that used by us in Oxford. The log ratio Potency Manchester/Potency Oxford was estimated to be $\bar{1}\cdot8091 \pm 0\cdot0624$, which gives a ratio of approximately 0·64. In terms of Oxford rats therefore this fine powder appears to have been at least as toxic as the first shipment.

Sample S 3. Received July 1943. Coarse powder. This shipment, tested at Manchester, appeared to be relatively inert, no deaths being recorded at doses ranging up to 1,250 mg. per kilo above which level it is not easy to test.

Sample S 4. Received October 1943. The shipment consisted mainly of coarse powder, a little fine was also sent but was not tested. Two preliminary tests on Oxford rats (T. 39, 40) suggested that the L.D. 50 of this sample was around 840 mg. per kilo. A further test on a small scale done in June 1944 (T. 48) indicated a similar figure. Combining these three tests together the L.D. 50 was estimated to be 805 (256–1,619) mg. per kilo. The comparatively large range of this estimate was in the main due to the low value found for the slope of the regression line, namely, $b = 2\cdot481 \pm 1\cdot164$, this value being only 2·13 times its standard error. When compared with the standard squill curve which was based on the tests made on sample S 1 between February 1943 and July 1944, it was found that whereas the slopes of the regression lines did not differ significantly ($\chi^2 = 2\cdot12$: $P = 0\cdot20$–$0\cdot10$), the differences in position were marked, $\chi^2 = 14\cdot13$ and $P = < 0\cdot001$. There seems to be little doubt therefore that this sample was definitely less toxic than the standard and it was used only in extract form.

Sample S 5. Received January 1944. This shipment consisted of both coarse and fine powders. The coarse powder which formed most of the material was tested on only a few groups of rats at Oxford. The results indicated a L.D. 50 of about 590 mg. per kilo, and no significant difference could be found between the regression line based on the two points available and the standard squill curve used at this time, the value of χ^2 for differences in position being 2·61 and P very nearly 0·10. A much fuller test on this coarse powder carried out at Manchester (M.R. 12) gave a L.D. 50 of 756 (664–859) mg. per kilo. However, this test was not carried out on their usual rat stock but on a number of animals which were specially bought for the purpose. The general indications from these tests were that this was a powder of a fairly high degree of toxicity and that it might even be as toxic as that of the first shipment. It was decided to recommend that the coarse grade should be released for use in the field.

The remaining part of this shipment consisted of a finer grade powder and a preliminary test at Oxford in August 1944 suggested that this was somewhat less toxic than the coarse grade, and that it had a L.D. 50 which was possibly in the region of 800–900 mg. per kilo. A further test of this fine powder was carried out much later in October 1945, and unfortunately it again had to be done on a small scale owing to a shortage of male rats. Taking the results of these two tests together, the L.D. 50 is in the region of 802 mg. per kilo. The apparent toxicity of this sample may be somewhat enhanced by the fact that the later test was done at a time when the rat stock may have become more sensitive to squill. In spite of this possibility, however, a comparison was made between the tests on the fine powder and the Oxford test on the coarse, since any change in the rat stock would be in the direction of underestimating any differences between the samples. For differences in position between the two lines $\chi^2 = 5{\cdot}56$ and P lies between 0·02 and 0·01. The log ratio of potency (fine/coarse) was $\bar{1}{\cdot}8594 \pm 0{\cdot}0477$ and the ratio was 0·723 with a range ($P = 0{\cdot}95$) of 0·927–0·563. This evidence suggests that the fine powder was only about three-quarters as toxic as the coarse.

Sample S 6. Received 1944. Coarse powder. The first test of this sample made during November 1944 (T. 57) gave a L.D. 50 of 829 mg. per kilo. However, the dosage mortality curve was extremely flat, in fact the slope of the regression line was $b = 2{\cdot}030 \pm 1{\cdot}371$, a value which is not significantly different from zero. Strictly speaking, therefore, no estimate of the L.D. 50 had been made at all. Later a parallel test was carried out in January 1945 and the standard squill sample S 1 was used as a basis of comparison, each sample being tested on the same day at three dose levels. For differences in position of the two lines $\chi^2 = 3{\cdot}86$, a value which is just, but only just, significant at the 5% level. Since there was no significant difference between the slopes, the ratio potency S 6/S 1 was estimated and found to be 0·479 (0·996–0·223). In round figures, therefore, the sample S 6 appeared to be only about half as toxic as the standard S 1, though this result was barely significant. The L.D. 50 of this second test (T. 66) on S 6 was approximately 890 mg., which is not very different from the rather indeterminate figure of 829 found in the first test (T. 57). Since the balance of evidence was in favour of this being regarded as a squill of only a moderate degree of toxicity, it was recommended that it should not be distributed for field work, but should be extracted.

If the two tests (T. 57, 66) are taken together, the L.D. 50 is 857 mg. with a range ($P = 0{\cdot}95$) of 323–∞ mg. per kilo. Although the symbol for infinity has been written for the upper limit, it is actually a relatively large figure of about 28 g. per kilo. The reason for this immense range is that the value of the slope of the regression line was low, and estimated with only a moderate degree of precision. The value found, $b = 2{\cdot}225 \pm 1{\cdot}118$, is almost precisely on the 5% level of significance, and in such cases one or other of the limits for the range becomes infinite (Irwin, 1943). The question of the variability between the slopes of the regression lines which have been fitted to these individual tests on the squill samples is discussed below.

Comparison between slopes of regression lines.—In the discussion of the results of the toxicity tests which were made at Oxford on these seven squill samples, it will have been noticed that in the case of two samples (S 4 and 6), both of only a moderate degree of toxicity, low values for the slopes of the regression lines were recorded. In surveying the results for squill as a whole it is of some interest to see whether these values of b were in any way abnormal. It does not appear that a low value for the slope was invariably associated with a L.D. 50 of around 800–1,000 mg. per kilo, for the regression line in the case of a Sicilian sample with a L.D. 50 of 883 mg. per kilo, had a slope of $4{\cdot}653 \pm 1{\cdot}275$, a value which is very comparable to those recorded for the standard sample S 1. Table 11 gives the appropriate figures from which an estimate of the average slope can be made for all the toxicity tests on squill for which individual estimates of the slope have been calculated.

The common slope estimated from the totals of the B and A columns is $b_c = 4{\cdot}0390$ and putting

$$\chi^2 = \Sigma\, Bb - b_c \Sigma B,$$

we have $\chi^2 = 10{\cdot}9$,

a value which is quite reasonable for 11 degrees of freedom. The slopes of the individual regression lines thus appear to have been consistent over the period of time 1941–5, during which these tests were done, and the average value is $b_c = 4{\cdot}0390 \pm 0{\cdot}4484$. It will be seen from Table 11 that this estimate is not very different from those recorded for the standard squill curves for sample S 1 which were obtained by fitting a single line to a number of separate tests on this sample. There is thus no evidence that the slope was affected by the change which took place in the animal stock after the autumn of 1944.

As regards the statistical analysis of these data, it would now be possible to recalculate all the L.D. 50's together with their respective ranges of error by employing this more accurate estimate of the common slope instead of the individual estimates of the slope in each case. The calculation is however rather a tedious one. The chief advantage in so doing would be in an improved estimate of the range of error, particularly in those cases where the individual regression line gave rather wide limits owing to a low value of *b*. The individual estimates of the L.D. 50 are not likely to be very greatly affected in the present series of tests. Although we have not troubled to recalculate the whole of the data, it was of interest to see very roughly the extent to which the ranges of error would be affected in the tests on squills S 4 and S 6, since the L.D. 50 for each of these samples was not very accurately estimated from the individual lines. Since both of these slopes were less than the average slope for the whole series of tests, the use of the latter as a provisional line would result in a somewhat lower total for the weighting coefficients and therefore in increased variance of the weighted mean of the probits. A new series of weights was therefore obtained in both cases. Since the value of the average slope is some 9 times its standard error, the approximate method of determining the range will be sufficiently accurate for our present purpose. The results were as follows:

TABLE 11. *Comparison between slopes of regression lines for red squill powders*

Squill sample	*Test*	$B = Swy(x-\bar{x})$	$A = Sw(x-\bar{x})^2$	*Regression coefficient b*
S 0	T. 002*a*	1·25254	0·37465	3·3432
S 0	T. 002*b*	1·65755	0·32499	5·1004
S 1	T. 20, 21	3·10793	0·48910	6·3544
S 1	T. 43	3·04512	0·69211	4·3998
S 1	T. 61, 62	1·76287	0·26490	6·6549
S 1	T. 64	0·57214	0·16342	3·5010
S 1	T. 65	1·39017	0·38732	3·5892
S 4	T. 39, 40, 48	1·83221	0·73855	2·4808
S 5	T. 46	0·82052	0·11907	6·8911
S 6	T. 57	1·07895	0·53153	2·0299
S 6	T. 66	0·70273	0·27197	2·5839
Sicilian	T. 003	2·86296	0·61532	4·6528
		SB = 20·08569	SA = 4·97293	

		Range % (P = 0·95)	
Squill sample	*Tests*	*From individual lines*	*Approximate values, using average value of b*
S 4	T. 39, 40, 48	32–201	79–126
S 6	T. 57, 66	38–∞	83–120

It is evident from the figures in the last column that the accuracy of the estimates would be improved considerably by the use of the average slope. This improvement, which is marked in the case of these two squill samples, would however be very much less in those other samples, where the L.D. 50 has already been estimated from the individual line with a reasonable degree of precision.

(iii) *Geographical variation*

Nothing is known of the toxicity of squills from French African possessions, other than Algeria, which border on the Mediterranean and Atlantic except the remarks by Danzel (1935) that they are toxic and the brief paper by Gattefosse (1944) which states that Moroccan bulbs are toxic.

Mediterranean areas.—Toxic bulbs are said by Danzel (1935) to occur in Spain and Portugal. In October 1941 a sample was received from Portugal and tested by the Bureau. (It was assumed, though not proved, that this squill had actually grown in Portugal.) The sample consisted of some dried squames which were whitish-pink in colour, and some pink powder of a mesh size similar to that of coarse S.A.L.A.M. material. The powder was tested on male white rats and failed to produce symptoms or death at doses up to 1·5 g. per kilo. A further test conducted by British Drug Houses Ltd. confirmed these findings, but a single male rat was killed with a water suspension given by stomach tube at a dose level of 3·2 g. per kilo, and another at 4·0 g. per kilo. A fresh squill bulb from Portugal showed red scales only in a thin outer layer, the inner ones being white.

Danzel (1935) states that the inhabitants of the Balearic Islands use native squill for rat killing. Munch, Silver, and Horn (1929) conducted a long series of tests with powders of Sardinian origin. The bulbs varied from white to dark mahogany red. The resultant powders showed various toxicities but included material of high toxicity from this source. The authors also show quite clearly that bulbs with only faintly pink inner scales may be highly toxic, one of this colour killing at 250 mg. powder per kilo. Mann (1937) also quotes Sardinia as a source of toxic powders. Danzel (1935) indicates that the species does not occur in Corsica. Powders from Italian and Sicilian sources are known to be toxic and have been largely used in U.S.A. (Danzel, 1932) and Germany (Mann, 1937). de Mori (1931) states that red forms, which have been exported particularly to the U.S.A., are found in southern Italy and Sicily. Flaccomio (1934) also notes that Italian squill has been used in rodent control. A sample of Italian origin, received by the Bureau in 1942 from the Department of Agriculture for Scotland, had come from the Admiralty Marshal in 1940. It was stated to be from Sicily, but it has been suggested that it may have been grown in some other part of Italy and only sun-dried in Sicily. It was a pale pink powder of coarse texture. From a test on male white rats (T. 003) a L.D. 50 of 883 (675–1,085) mg. per kilo was estimated.

Both de Mori (1931) and Danzel (1932) have recorded that toxic red bulbs occur in Libya. A small sample received and tested by the Bureau from Tunis in 1945 was inert at doses up to 1,200 mg. per kilo for male white rats.

Squills in Malta and neighbouring islands are practically all white. Red bulbs, rather pale red, do occur but are rare. The Director of Agriculture for Malta has stated that such red bulbs have been tested and found non-toxic. Winton (1927 *b*) tested white bulbs on male white rats and failed to produce any toxic symptoms at doses up to 4 g. per kilo of a substance which had been processed in order to concentrate any toxic principle.

Egypt.—Knowledge of the toxicity of Egyptian materials is far from complete, but it is sufficient to indicate that toxic samples can be obtained though others may be useless. Bulbs of red or pink colour occur all along the Egyptian littoral, where the white form is also found. Kamal and Hefny (1941) produced toxic effects both with pink and yellowish powders from scales of the same bulbs. The bulbs were gathered at Rafa in the Sinai peninsula on the Palestine border. Although they give no figures from which a L.D. 50 can be derived, they killed twenty-four out of twenty-five wild rats in cages by feeding 4% powder in baits. They state, without giving the details, that the yellow powder is more toxic than the pink. A sample of powder was prepared by the Pharmaceutical Society from a fresh Egyptian bulb received in 1934 but the exact source was not known. The dried scales were sorted into two lots, more or less coloured, before grinding. With each of these powders, given in water suspension by stomach pump, five out of eight rats died at a dose level of 1 g. per kilo. Bulbs were received by the Bureau from the east and west sides of the Nile delta in 1940. The Director of the Fûad I Agricultural Museum stated that those from the west bank were darker, those from the east having only the outer squames pink, the inner being white. These bulbs were oven-dried for about 24 hours at 65–70° C. and then ground. The resulting powders varied in colour from pale sandy buff to pink. None of these powders produced illness or death at doses up to 2 g. per kilo. A further sample of the reddest bulbs was available from the west of the Nile later in 1940. These were tested both fresh and as powder derived by the same process as before. Fifty rats tested at doses up to 1·50 g. powder per kilo showed no illness. A further test killed one rat at a dose of 1·85 g. per kilo, and one at 2·3 g. per kilo, although three rats which received 3·5 g. per kilo were unharmed. Powder derived from five of these bulbs, which had been dried at a lower temperature 45–52° C., also failed to kill at doses of 3·50 g. per kilo.

Cyprus.—The plant is common throughout Cyprus, and the Director of Agriculture for Cyprus (1934 in litt.) has stated that all shades occur from white to intensely deep red. Toxicity tests described by Claremont (1922), but without detail, showed that powder from red scales killed at least one rat at 1 g. per kilo, though lower doses had no effect. Powder from yellow scales killed at 2 and 3 g. per kilo. Further samples from Cyprus were tested by Winton (1927 *b*), who states that 'the powder prepared by drying these [pinkish scales] failed to kill rats in doses of 0·50 gram'. Wokes and Willimot (1934) have also published findings both on the cardiac and the rat-toxic principles in Cyprus squill. Their findings agree in general with those of Claremont that doses of less than 1 g. per kilo were harmless; a few deaths occurred at 1 g. per kilo and nine out of eleven rats died at 2 g. per kilo. Three samples of sun-dried scales were received by the Bureau in 1940; they were described as 'very dark red', 'lighter red', and 'faintly red'; after being ground, the palest one was yellowish. Tested on male white rats, a mixture of the three powders in equal amounts produced illness only at doses up to 2 g. per kilo.

Palestine.—The only sample of Palestinian squill tested was a powder received through I.C.I. Ltd. in 1941. It was derived from squames which had been partially sun-dried and partially dried in a native oven 'kept at a moderately low temperature'. The fresh bulbs had been collected just south of Gaza near the Egyptian border and presumably within a few miles of the source of the bulbs used by Kamal and Hefny (1941). In two tests only one rat died, at a dose of 1·53 g. per kilo, five others which had received doses from 1·15–2·30 g. per kilo were ill. Others receiving doses up to 3·50 g. per kilo were unaffected. Green (1944) has noted that a study of the toxicity of native red bulbs is being made at the University of Beirut, Lebanon.

TABLE 12. *Scilliroside content (after Stoll and Renz 1942 a)*

	Flowering time Aug./Sept.		*Dormant period after flower and before leaves*		*Leaf period April*	
Source	*Scilliroside content mg. per kilo fresh bulb*	*L.D. 50 powder equivalent mg. per kilo of rat*	*Scilliroside content mg. per kilo fresh bulb*	*L.D. 50 powder equivalent mg. per kilo of rat*	*Scilliroside content mg. per kilo fresh bulb*	*L.D. 50 powder equivalent mg. per kilo of rat*
Cherchell	370	400	..	..	10	14,000
El Milia	..	..	60	2,300	Trace	..
Miliana	170–200	825–700	..	..	40–70	3,500–2,000

Apart from wide variations over the Mediterranean area it is clear also that there may be marked variations in toxicity in bulbs from adjoining localities. Table 12, which is adapted from that of Stoll and Renz (1942 *a*) indicates this geographical variation in Algeria. All three localities are in the eastern coastal strip, Miliana being just south of Cherchell, and El Milia about 200 miles farther east. A similar difference is perhaps shown on the Palestine–Egyptian border south of Gaza, described under these two countries above, though the evidence here is far less satisfactory.

(iv) *Colour*

It will be seen from the foregoing section that the problem of toxicity in relation to colour is not satisfactorily settled. While, in general, it is clear that the toxic forms are redder and that the deeper the form the more likelihood there is of its being toxic, the evidence of Munch, Silver, and Horn that whitish-pink Sardinian material could kill at 250 mg. per kilo, and the findings of Claremont in Cyprus, and Kamal and Hefny in Egypt, indicate that even yellowish bulbs can be toxic to rats.

(v) *Season*

Danzel has been careful to point out the great importance of harvesting the bulbs at the correct season. This, he says, varies to some extent from year to year according to the growth of the plants and the weather; but it is always somewhere in the dry period from July to October. Bulbs collected in winter and spring, when they are very fleshy and bearing leaves, are always inferior. Danzel does not give any toxicity figures in support of his general statements, but most interesting corroborative evidence is found in Stoll and Renz (1942), who have studied the seasonal difference in scilliroside (rat-toxic principle) content of fresh bulbs collected in Algeria. Table 12 gives figures (no details of toxicity tests are published, so the range of error is unknown), and their conversion into the approximate equivalent L.D. 50 for dry powder, for three localities, and for each of them at two different seasons. The August–September toxicity for Cherchell (the site of the S.A.L.A.M. stablactivation factory) agrees closely with that of our sample S 1, while that for the same place in April is extremely low, and would have been classified by us as practically inert. The relative toxicities of bulbs from other areas at these two seasons show similar, though less great difference. It is probable that variations in the toxicity of different shipments from Algeria, summarized in Table 10, may have been partly due to seasonal factors.

(vi) *Processing*

Winton's work (1927) had suggested a correlation between toxicity and fineness of texture of red squill powder. A sample of the coarse material (sample S 2) was therefore ground to finer texture, the specification being that practically all particles should pass through a 60-mesh sieve. The original fine grade of powder could not be sieved very accurately because of caking in the barrels, and the clinging together of the finer particles; but less than 1% was held by a 40-mesh sieve and the majority was much finer. A test of toxicity on Manchester rats showed no improvement in toxicity as a result of the grinding.

Danzel has stressed in many of his papers that the 'stablactivation' process developed by him not only preserves the full toxicity of the bulb in the powder produced, but gives these powders keeping qualities that are not possessed by powders derived from other drying processes. On the first of these points it is clear that a good sample of stablactivated powder is as toxic as any powder is likely to be, but, on the other hand, the work of Munch, Silver, and Horn and other American authors has shown that oven-dried or sun-dried powders can be just as toxic. No absolutely comparable tests are available, however. These authors discuss the question of the effect of temperature on toxicity and, in a series of admittedly incomplete experiments, conclude 'that the most toxic powders were produced at a drying temperature of 80° to 100° C'. A lower temperature, 60–70° C., has been used by Danzel (1932). Munch, Silver, and Horn also show that fermentation of the fresh squames decreases the toxicity.

(vii) *Age*

Details of tests on keeping qualities of sun- and oven-dried powders have not been traced, but it has been repeatedly stated that samples do lose their toxicity on keeping. This deterioration is mentioned for instance by Raebiger (1931), Loir and Vigné (1931), and Chodzko (1932), in relation to experiences with powders in the field. Danzel's claim that stablactivated powder retains its toxicity more or less indefinitely has been substantiated by tests over a period of 25 months by the Bureau on sample S 1. These are described in detail above, and it will be seen that a L.D. 50 of 357 mg. per kilo was found in a combination of tests carried out when the powder was between 22 and 25 months old; this figure compares very closely with, and is well within the range of, that of 419 mg. per kilo found when the shipment was first received. It may be noted that Claremont (1922) has recorded a deterioration in powders derived from *Urginea Burkei*, which became completely inert after 8 months' keeping. It is probable that powders should be kept away from light, damp, and extremes of temperature.

(*d*) **Toxicity of extracts**

Brief notes have already been given in the section on extraction, about the types of toxicity found in the extracts reported in the literature; it remains only to summarize the findings on the extracts prepared by the Bureau. Ten tests were carried out on the Bureau stock of white rats, eight on males and two on females; and two further tests were carried out on a rat stock of different origin. These tests are not sufficient to give an accurate L.D. 50 in any individual case, but if all the tests with males are summed, it is found that twenty out of forty-seven were killed at a dose of 212–20 mg. extract per kilo, a mortality of 42·5%. These tests are summarized in Table 13. If the toxicity had been fully extracted in a 24-hour extraction, the L.D. 50 of the extract should be about 160 mg. per kilo, since 37% by weight of extract was derived from a powder with L.D. 50 of about 440 mg. per kilo. The L.D. 50 of the extract is clearly not as low as this. It should be noted that the extract, like the powder, is considerably more toxic to female than to male rats.

TABLE 13. *Toxicity of laboratory extracts of red squill*

Test No.	*Extr. No.*	*Extraction time (hrs.)*	*Extract dose (mg. per kilo)*	*Powder (mg. per kilo)*	*Mortality/ total rats*	*Sex*	*Notes*
1	1	6	44	200	1/5	♂	
			110	500	0/5	♂	
			220	1,000	3/5	♂	
2	1	6	220	1,000	7/11	♂	Survivors from test No. 1.
3	4	6	220	1,000	2/14	♂	Survivors from a test with extract No. 3.
4	4	6	330	1,500	3/4	♂	Survivors from test No. 2, 18 days later.
5	6	8	214	1,500	8/12	♂	Survivors from test No. 3, 14 days later
6	14	8	16	100	0/5	♂	
			32	200	0/5	♂	
			64	400	0/5	♂	
			128	800	0/5	♂	
7	3	12	53	200	0/4	♂	
			132	500	1/5	♂	
			212	800	0/5	♂	
			265	1,000	4/5	♂	
8	10	24	40	100	6/7	♀	
			80	200	7/7	♀	
			160	400	7/7	♀	
9	12	24	19	50	0/10	♂	
			38	100	0/10	♂	
			76	200	1/10	♂	
10	12	24	76	200	6/10	♀	
11	12	24	76	200	8/9	♂	These rats were from a different stock.
			152	400	9/10	♂	
12	13	24	76	200	8/10	♂	
			152	400	8/10	♂	

Two toxicity tests were carried out on the bulk extracts, one on the cold extract and one on the hot. These tests (Table 14) show a very similar L.D. 50; that for the cold being 280 mg. per kilo, and that for the hot 220 mg. per kilo., for male white rats, the extract being expressed as total solids. If the toxic principle had been fully extracted the L.D. 50 of the cold extract should be about 200 mg. per kilo, and that of the hot about 220 mg. per kilo, since 24% and 26% by weight respectively were derived from a powder having a L.D. 50 of 805 mg. per kilo.

At the conclusion of the experimental work on bulk extractions, it was recommended that some water should be added to the commercial extract before distribution to the user. The object of this step in the process was to provide a standard watery extract for the user, and to avoid having to issue the separate instructions that would be needed for the pure, undiluted extract, which would vary in toxicity from sample to sample according to the source of the powder, &c. It was found, however, that when this watery extract was allowed to stand, a gummy deposit separated out on to the bottom of the container, which was not easy to stir back into the liquid before use and which the pest operators were tending to throw away. Tests were therefore undertaken to see how much of the toxic components settled out in this gum, using commercial extract to which 50% water had been added, and which had been allowed to stand undisturbed for nine days. These tests are summarized in Table 15. The L.D. 50 of the whole extract is about 0·48 ml. per kilo body weight, and that of the supernatant liquid about 1·19 ml. per kilo. Tested statistically, these two tests differ significantly, for the value of χ^2 for differences of position of the two regression lines was 18·42 (1 d.f.) and $P = <0{\cdot}001$. The log (potency of supernatant liquid/potency of whole extract) was estimated to be $\bar{1}{\cdot}6195 \pm 0{\cdot}0780$, which gives a ratio between the potencies of 0·416, with a range of error

TABLE 14. *Toxicity of bulk extracts of red squill to male rats (T. 45, 47)*

Extract dose (mg. per kilo solid extract)	*Powder dose (mg. per kilo)*	*Mortality/total rats*	*wt. range (g.)*	*Date*
	Extract No. E. 21 derived from cold extraction			
170	625	0/5	150–245	9 April
340	1,250	4/5		
680	2,500	5/5		
1,360	5,000	4/4		
	Extract No. E. 22 derived from hot extraction			
156	600	3/5	100–205	7 June
312	1,200	3/5		
624	2,400	4/5		
1,248	4,800	5/5		

TABLE 15. *Toxicity of extracts of red squill which have been allowed to stand (Male rats, T. 80, 79; 20 April 1945)*

Dose	*Mortality*	
c.c. per kilo	*'Whole extract', stirred up*	*Supernatant liquid no stirring*
0·15	0/10	..
0·3	1/10	0/10
0·6	8/10	1/10
0·9	9/10	3/10
1·2	..	5/10
wt. range (g.)	160–300	150–290

($P = 0{\cdot}95$) of 0·261–0·576. In round figures, therefore, the supernatant fluid was only about $\frac{5}{12}$ as toxic as the whole extract, the value of this ratio lying roughly between $\frac{3}{12}$ and $\frac{7}{12}$.

(*e*) Field tests

Because of its relatively low toxicity red squill powder is only used in a wet base, which rats eat more rapidly than dry ones (Ch. 7). Most tests were carried out with 10% powder in 1:1 rusk, plain or with sugar, and gave several excellent results. Often, however, only about 75% of the population was killed, and there was a suggestion that a higher concentration or wetter base might have given more consistently good results. Red squill extract in rusk gave poor results at 5% powder equivalent, but at 10% results were about as good as with any inorganic poison (Ch. 4).

(*f*) Symptoms and post-mortem findings

White or wild rats poisoned in the laboratory show very characteristic symptoms immediately distinguishable from those following any of the other standard poisons used in rat control. These symptoms begin to appear about 2–4 hours after the consumption of a lethal dose and take the form of a general paralysis, often starting with the hind limbs, followed by rolling over and over on the long axis. The animal may attempt to rise on its feet and will then fall over on its side or roll completely round; some animals roll always on one side while others roll either way. Between and during rolling spasms the tail is often moved in a circular flailing manner. These symptoms, characteristic of a nerve poison, are accompanied by violent diarrhoea and excessive urination, and continue spasmodically until death, which usually occurs within 1–2 days, but may take place as much as 5 days later. Death probably takes place from respiratory paralysis. The effects of powders and extracts are the same, except that, for comparable doses, the extracts produce symptoms earlier and death occurs more rapidly. If the external behaviour of a rat poisoned with red squill is considered in anthropomorphic terms, it indicates a very painful, prolonged, and dirty death, and if cruelty is a consideration red squill should never be used.

In colonies of wild rats which have been poisoned with red squill baits the number of corpses found is, as with other poisons, very variable and generally less than half the total. The usual proportion is somewhat higher than with arsenious oxide but lower than with zinc phosphide, though the extract, especially, may yield comparably high numbers (Ch. 4). Perhaps the greater speed of action of the extract compared with the powder makes more rats die on the surface. Red squill, because of its scouring and dehydrating effect, is sometimes considered to reduce the chances of 'corpse smell'; as with arsenious oxide this may be true under some circumstances, but is undoubtedly not always so.

(*g*) Danger

(i) *General*

The great reputation held by red squill as a rat poison is based almost entirely on the belief that it is harmless to man and domestic animals. This supposition has been used extensively by manufacturers of rat poisons containing red squill in their advertising matter. Such statements as 'harmless to domestic animals, pets and poultry', 'harmless to human beings', occur frequently. Others are more cautious and use the words 'safe' or 'comparatively harmless', thus following but not elaborating on the words of the M.A.F.'s Bulletin No. 30 (1925–45). A substance which, if ingested, gives rise to such symptoms as vomiting, diarrhoea, nervous spasms, and may even cause death in man, domestic animals, and poultry, can scarcely be called harmless: it can, however, be called relatively safe and it is pertinent to review the history of this belief and discuss the suppositions on which it is based before analysing the available evidence.

As has been pointed out in the introduction, the squill bulb has been known to possess toxic properties, separate from the properties of its heart glycoside, since at least the fourteenth century, and fatal case histories for man were detailed in the eighteenth century. Orfila (1814–15) showed its fatal toxicity to dogs under experimental conditions and Cornevin (1887) has given lethal doses for various domestic animals. No mention has been found of its supposed rat specificity until the beginning of this century, when it was first coming into popular use as a rat poison. The Danish Rat Law (1907) recommends its use on the grounds of safety, obviously basing such recommendation on earlier evidence, which has not been seen. From then onwards, though not necessarily derived from this recommendation, the belief has become widespread, and remains unshaken in spite of a considerable amount of published work in direct contradiction to it. The supposition is based on a mixture of three beliefs: firstly, that domestic animals will not eat red squill in baits; secondly, that if they do eat it they will vomit and not suffer further; and thirdly, that if they do eat it and do not vomit they will suffer no ill effects. It is undoubtedly true that cats, dogs, pigs, sheep, and cattle are very reluctant to eat red squill powder in baits even in low doses. The bitter taste is very noticeable to the human palate and baits would probably not be eaten willingly; but chickens take it readily. Sheep, cattle, and chickens, like rats, cannot vomit; horses do so with great difficulty, often with serious damage to the stomach wall. Red squill causes vomiting almost invariably in man, dogs, and cats and sometimes in pigs. Fatal cases of poisoning in man, pigs, sheep, cattle, horses, dogs, and chickens are known.

(ii) *Man*

The evidence of fatal or serious poisoning in man is not entirely satisfactory because it is not usually stated whether the squill was red or white;

and since no tests have been made for the presence of squill at post mortem, the chance of pharmacists' errors cannot be entirely excluded. It is clear, however, that the symptoms described in these cases are those associated with red squill poisoning or a mixture of these combined with those of the heart glycoside, the presence of convulsive spasms being particularly characteristic. This picture is given by Cazin (1868) of the effect of excessive doses of squill, red squill probably being referred to because it has always been preferred in the French Pharmacopoeia, 'ils produits des nausées, des vomissements, de la cardialgie, des coliques, l'hematurie, la strangurie, la superpurgation, l'inflammation et la gangrène de l'estomac et des intestins, des mouvements convulsifs, et la mort'. This author quotes two fatal cases, in one of which the dose was 990 mg., and one, from Roques (1821), which produced violent vomiting and spasms, though the patient recovered. Two further fatal cases and another serious one are given by Hughes and Dake (1890). M. Bertrand, of S.A.L.A.M., in a personal communication, has stated that early agricultural colonists in French North Africa cooked red squill bulbs and ate them like onions, with resulting illness and death. There are also cases on record, such as one in Nottingham (Anon. 1886), of death and illness which are clearly due to the heart glycoside alone. Two other characters of red squill which may cause irritating symptoms to man must be noticed. The juice of the fresh bulb, if rubbed on to the skin gives rise to reddening and even blistering; this is probably due to the abundant oxalic acid raphides and applies equally to red or white bulbs. Red squill powder, particularly if finely ground, will cause violent and persistent sneezing and, if large quantities are being handled, a dust mask should be worn.

(iii) *Pigs*

Pigs are certainly susceptible to red squill poisoning; Rietz and Moore (1943) give details of a case in which forty-six pigs died from eating swill to which had been added sweepings of hamburger rat bait. Their diagnosis was based on symptoms, post-mortem findings, and circumstantial evidence. Brüning (1937) refers to a case in which six pigs died after eating red squill baits prepared for rats. Hautsche (1933) mentions a further two. Cornevin (1887) says that in Algeria hungry pigs will eat red squill bulbs and die. de Mori (1931), presumably referring to Italian squill, says that cases of pigs poisoned by eating the bulb have been verified. Two series of toxicity tests are known, one in some unpublished research by Winton for the M.A.F. in 1926 briefly reported by Read (1931) on four pigs, one of which refused to eat the bait, and one by Gwatkin and Plummer (1943) at five dose levels. To these can be added four pigs dosed with red squill by Gwatkin and Moynihan (1944) in order to produce gastro-intestinal irritation and one treated by Munch, Silver, and Horn (1929). The toxicity to rats of the powder used in the Ministry's test is not given; the toxicity

TABLE 16. *Toxicity of red squill powder to pigs*

From (a) Gwatkin and Moynihan (1944); (b) Gwatkin and Plummer (1943); (c) M.A.F. (1926, unpublished)

Dose (mg. per kilo)	*weight (kilo)*	*Sex*	*Method of administration*	*Time to symptoms*	*Time to death*	*Symptoms*	*Ref.*
1,000	15·4	..	Fed in 1 lb. oatmeal and 1½ gals. water	..	..	None, dull for a few days	*c*
400	8·2*	♀	Capsule	..	Next morning	..	*b*
400	14·0	♂	,,	..	,,	..	*b*
337	7·7	..	,,	..	,,	..	*c*
300	11·3	♀	,,	Next morning	5 days	..	*b*
300	10·6	♂	,,	Next day	..	Very ill with prostration, coldness and evidence of pain. Recovered in 5 days	*b*
250	29·0	..	Fed in 2 lb. middlings with water	..	..	None, dull for a few days	*c*
200	*c.* 22†	..	? Forced feeding	..	8 days	..	*a*
200	9·1	♀	In water by stomach tube	Next day	..	Inco-ordination and twitching; bad appetite	*b*
200	8·6	♀	,, ,,	,,	..	Inco-ordination and twitching; bad appetite	*b*
200	*c.* 22†	..	? Forced feeding	..	..	Severe gastritis and enteritis	*a*
200	*c.* 22†	..					*a*
200	*c.* 22†	..					*a*
200	6·4‡	♀	Capsule	..	..	Off feed for a short time	*b*
100	8·2*	♀	,,	..	..	None	*b*
50	6·4‡	♀	,,	..	..	,,	*b*

*, ‡ Two tests on the same animals. † Four animals in a batch of average wt. 22 kilo.

(L.D. 50) of the powder used by Gwatkin and his co-workers was about 600 mg. per kilo for white rats. The findings are summarized in Table 16, and except for a pig that was unaffected by 1,000 mg. per kilo the figures show that doses over 200 mg. per kilo are usually fatal and that one death occurred at this level. Other doses of 200 mg. per kilo all produced serious symptoms. It should be noticed that all the pigs used in these tests were immature. Cornevin (1887), without detail, gives a lethal dose of 250 mg. per kilo. Hertwig (in Fröhner, 1901) records diarrhoea in pigs which received 2 g. of fresh bulb.

In these same papers there is evidence that pigs will show the greatest reluctance to eating poisoned food. In that of the Ministry it is stated that 'in spite of the fact that they were given no other food, it was found almost impossible to induce . . . pigs . . . to eat any appreciable quantity of poisoned feed'. Gwatkin and Plummer carried out some detailed tests on refusal, by offering hungry pigs their normal food, wet and dry, with about 6% (1 oz. to 1 lb.) of red squill added. Out of eight animals only one ate the food at all and that one but very little. Rietz and Moore (1943) point out that garbage-fed pigs are accustomed to eating mixtures not readily taken by other animals, so that the refusal safeguard may not be effective in such cases.

(iv) *Sheep, cattle, and horses*

For sheep, cattle, and horses the information is much more scanty. The only field evidence known is that given by Cornevin (1887) and by de Mori (1931), that young ruminants may take it, in pastures, with subsequent poisoning. Danzel (1931) observes that herds which graze on fresh squill leaves are 'incommoded'. In the series of tests by the Ministry referred to above, two sheep were used; one, a male of 37·6 kg., died after being drenched with red squill in oatmeal and water corresponding to a dose of 500 mg. per kilo. The other, a female, consistently refused to eat the bait. Five calves were also tested, one of 174·6 kg. died after being drenched with red squill in oatmeal and water corresponding to a dose of 1,000 mg. per kilo. Two others, 165 and 108·8 kg., showed poison symptoms: the first, which was drenched, received a dose corresponding to 333 mg. per kilo; it was very ill but recovered in 6 days. The second, which ate the bait (oats, bran, and aniseed oil) received a dose corresponding to 125 mg. per kilo; it was only slightly ill with diarrhoea. Two others took an unmeasured small amount; one was slightly ill, the other unaffected. Hertwig (in Fröhner, 1901) produced diarrhoea in a cow with a dose of 30 g. of fresh bulb. The only known figures for horses are those given by Cornevin (1887), without details, of 200 mg. per kilo, and by Hertwig, who produced diarrhoea in a horse with 30 g. and killed two horses with 60 g. of fresh bulb. Cornevin also gives a figure of 500 mg. per kilo for ruminants in general, presumably meaning cattle, sheep, and goats, since he refers to horses and pigs separately.

(v) *Dogs*

The evidence for dogs is very scanty. Orfila (1814–15) showed on two dogs that, if vomiting was prevented, they died from doses of 72 g. of fresh bulb, with the usual nervous symptoms. Hertwig produced vomiting and diarrhoea with doses of fresh bulb ranging from 250 to 2,000 mg. and death with 100–500 mg. of an extract 'scillitoxin'. Abderhalden (quoted by Claremont, 1921–2) gives a lethal dose of 1 mg. per kilo of an extract 'scillain'. Gross (1940) offered an extract which had a toxicity to rats of 3–5 mg. per kilo, to a dog of 15·9 kilo; after taking several bites it refused to eat. It exhibited salivation and staggered a little the next day. The same animal, given 48·4 mg. per kilo of the extract by stomach tube, salivated intensely, vomited, had severe convulsions, and died in 27 minutes. M. Bertrand, of S.A.L.A.M., in a personal communication, has recorded the death of several dogs in Algeria which ate red squill baits prepared for rats. Munch, Silver, and Horn (1929) found that dogs would not eat meat containing 0·1–0·5% squill powder, but they also record two cases of dogs gulping sausage rat baits containing 10% red squill. Emesis was the only effect noticed. Red squill powder injected into the stomach by stomach tube produced vomiting and diarrhoea with doses of 100 mg. per kilo.

(vi) *Cats*

For cats Hertwig (in Fröhner, 1901) records vomiting and diarrhoea in animals receiving 250–2,000 mg. of fresh bulb, and Abderhalden (in Claremont, 1921–2) found that 2 mg. per kilo of an extract 'scillain' was fatal. Munch, Silver, and Horn (1929) found that meat containing 100 p.p.m. or less of red squill was taken fairly readily without ill effects, but that higher concentrations proved unpalatable; at 1% only 7 g. of bait was taken. Two tests carried out by the Bureau confirm these observations that red squill, as powder or extract, is very unpalatable to cats. The powder used had a L.D. 50 to male white rats of 420 mg. per kilo and the extract a L.D. 50 of 220 mg. per kilo. An experiment was carried out on six cats, giving powder to three and extract to three; the poison being given in the normal laboratory diet of chopped meat. The results are given in Table 17, and it will be seen that these otherwise attractive baits were either completely or almost completely refused by all the animals. Profuse saliva poured to the floor in strings.

A further experiment was carried out to see whether cats would eat rats which had been poisoned with red squill. These tests, which are summarized in Table 18, were intended to simulate the behaviour of farm cats which catch and eat dying rats. The same six cats were used. All six were prebaited for 1 day with freshly killed rats opened ventrally. These were completely eaten, and on the second day three cats were given freshly killed rats, opened ventrally, which, 5 hours before, had eaten a full meal of 10% powder in rusk and water 1 : 1 by weight. The other three cats received similar rats which had taken 10% extract soaked into bread. It will be seen that cat No. 21 whose rat had eaten the largest dose refused to eat at all; cat No. 25 whose rat had eaten the second largest dose ate all but the stomach and duodenum, where presumably most of the poison was still situated. This is particularly interesting because these laboratory cats nearly always ate the whole of the viscera of a rat first and seemed particularly fond of the stomach. The other four ate the whole of their rats, three vomited, one did not, and none was harmed.

TABLE 17. *Toxicity to cats of red squill powders and extracts*

Cat. No.	*Sex*	*Wt. (kg.)*	*Poison*	*Dose in g. per kg. body wt.*	*Effect*
21	♀	2·0	Powder	0·5	Refused to take bait.
22	♂	3·3	,,	1·0	Tasted, salivated heavily, and refused further bait.
23	♂ (cast.)	3·0	,,	1·5	Tasted, salivated heavily, and refused further bait.
24	♀	2·7	Extract (Alc.)	0·5	A little bait eaten, no salivating; cat well next day.
25	..	2·6	,,	1·0	Refused to take bait.
26	♀	2·7	,,	1·5	Tasted, salivated heavily, and refused further bait.

TABLE 18. *Toxicity to cats of rats poisoned with red squill powders and extracts*

Cat No.	*Sex*	*Wt. (kg.)*	*Wt. of rat (g.)*	*Poison*	*Dose in g. per kg. body wt.*	*Effects*
21	♀	2·0	220	Powder	0·55	Rat half eaten and stomach intact; cat unharmed.
24	♀	2·7	100	,,	0·15	Rat eaten, no vomiting; cat unharmed.
26	♀	2·7	125	,,	0·15	Rat eaten, but vomited a few hours later; cat unharmed.
22	♂	3·3	60	Extract	0·09	Rat eaten, but vomited a few hours later; cat unharmed.
23	♂ (cast.)	3·0	180	,,	0·27	Rat eaten, but vomited a few hours later; cat unharmed.
25	..	2·6	95	,,	0·31	Rat eaten, except stomach and several inches of duodenum. No vomiting; cat unharmed.

An interesting case of apparent immunity has also been observed in the field. At a heavily infested farm, three cats and two kittens were observed catching and eating sick rats which had been poisoned with red squill powder. Five partly eaten rats were found and a cat was seen with a rat in its mouth. One cat, with a litter of just weaned kittens, was seen to bring them a rat. No harm came to any of the animals.

(vii) *Poultry*

A number of toxicity tests have been carried out on chickens, and they all agree substantially that these birds are very resistant to the action of red squill. The presence of powders or extract in baits has usually been found to put them off their food to some extent, but this refusal is not nearly so marked as it is with other animals. Munch, Silver, and Horn (1929) have produced the most conclusive results, although unfortunately they do not state the exact toxicity to rats of the Sardinian powder which they used. It may be assumed, however, to have been of a fairly high order. They state that:

> no effect was observed following injection into chickens' crops of quantities less than 2,000 milligrams of squill powder per kilo of body weight. Diarrhea followed the injection of 2,000 and 3,000 milligrams per kilo. Squill powder was mixed with cracked corn and with laying mash in 10 per cent concentrations. After one or two picks, chickens refused to eat such food. Two chickens were placed on a diet containing 10 per cent of squill powder in growing mash. After refusing to eat for several days, they consumed the mash without much hesitation. This food was continued for two weeks, during which time the chickens apparently grew as well as controls fed on untreated mash.

These authors also quote Stewart as concluding that even in one, two, or three feedings without alternative food, baby chicks are not likely to be killed by feed containing 10% of powder.

Hamilton (1940) states that repeated trials with pullets indicate that squill, mixed in the proportion of one part to sixteen of mash, has no visible effect on the birds nor does it lessen their appetitite for mash so treated. The detailed evidence is not given but it may be calculated that the dose that would be received by a pullet of 1·4 kg. if it ate 113 g. (4 oz.) of mash would correspond to 4·7 g. per kilo. A further series of experiments was conducted by Winton for the M.A.F.: these have not previously been published but we have very kindly been allowed to summarize them. The toxicity to rats of the sample used was certainly above 500 mg. per kilo to males and was probably the same as that of a sample which had an A.L.D. of 1·17±0·07 g. per kilo. It was given by gelatine capsule to three groups of seven birds each, at dose levels of 2, 4, and 6 g. per kilo. At the lowest dose no symptoms were shown except that the droppings were greenish the following day. At the middle dose three birds appeared a little dull for a day or so. At the highest dose six of the seven birds died after being very ill for 4–8 days; the seventh was killed after being ill for 18 days.

A series of experiments on the effect of powders and extracts was conducted by the Bureau using the facilities of the A.R.C. research station at Compton, Berks. In the powder experiments (Table 19) twelve Rhode

Island Red × Light Sussex pullets were used: 3 months old and of an average weight of 1,420 g. (3 lb. 2 oz.). They were placed in individual indoor cages after having been on free range and a diet of meal mash and grain. Water was provided throughout. The powder used was sample S 1, which had a L.D. 50 to male white rats of 443 mg. per kilo. In the first test the birds were given 4 days' prebait with one part by weight of water to two of rusk (1:2 rusk) with no alternative food supply. This was followed by powder in 25 g. rusk corresponding to doses from 250–1,250 mg. per kilo. All except one bird ate the whole bait and none showed any symptoms. Three days later six of the birds were fed a surplus of 10% powder in 1:2 rusk and though total takes were slightly reduced there was no hesitation in accepting the bait. The maximum dose of powder by the following day corresponded to 8·27 g. per kilo. None of the birds showed any poison symptoms and they continued to feed normally on the following 2 days. The remaining six birds, Nos. 7–12, were fed 10% powder in meal mash on the same day as the other six. They showed no hesitation in accepting the bait, the maximum take by the following day corresponding to 9·49 g. per kilo. One of the birds (No. 9), which was not feeding well throughout the test, showed general external signs of illness the following day, and refused to eat unpoisoned mash on the following 2 days. It did not die but recovered when put back on to normal diet. The other five showed no poison symptoms and continued to feed normally. In the second parts of these tests there was a small error because the birds always scraped a small amount of food, poisoned or not, from the feeding trays on to the floor of the cages. All the birds lost weight during the tests, but this loss may be attributable to the change of diet and environment rather than to the poisoning.

In the experiments using extracts (Table 20) twelve Light Sussex cockerels, 10 weeks old and of an average weight of 1,200 g., were used under the same conditions as in the experiments with powder. In a preliminary test, after 2 days' prebait, the birds were given the extract in 25 g. meal and water at doses ranging from 250–2,000 mg. per kilo. The extract used was from samples E 8 and E 10, prepared by Soxhlet with methyl alcohol as a solvent, and having a toxicity of about 200 mg. per kilo to male white rats. All the birds except one took the whole bait, but none of them showed any ill effects. In a second test (Table 20) six of the birds were prebaited for 3 days with 1:2 rusk and after a day's starvation were given 10% of the same extract in the same base. The largest dose eaten by any bird corresponded to 10·8 g. extract per kilo and the smallest to 2·44 g. per kilo. All the birds had watery brown diarrhoea on the following day and were damp around the rump, crouched up, and inactive; by the second day they had regained much of their accustomed activity but the droppings were still softer than normal. On both these days they emptied their water bowls completely, whereas on the prebait days they had filled them

TABLE 19. *Toxicity of red squill powders to chickens*

Experiment 1 = Days 1–7. Birds 1–12
Experiment 2 = Days 8–10. Birds 1–6
Experiment 3 = Days 8–10. Birds 7–12

			Day										
			1	*2*	*3*	*4*	*5*	*6*	*7*	*8*		*9*	*10*
No. of bird	*Original weight of bird (g.)*	*Final weight of bird (g.)*	*Takes of 1 : 2 rusk (g.)*				*Dose (mg. per kilo)*	*Takes of 1 : 2 rusk (g.)*		*Takes of 1 : 2 rusk or meal mash + 10% squill (g.)*	*Dose (g. per kilo)*	*Takes of meal mash (g.)*	
										Rusk			
1	1,330	1,300	115	125	150	150	250	155	150	105	7·89	130	150
2	1,530	1,450	70	130	150	150	250	115	130	75	4·90	145	145
3	1,390	1,360	75	95	125	130	250	90	100	70	5·04	110	130
4	1,270	1,250	95	140	150	150	500	200	150	105	8·27	150	150
5	1,640	1,580	55	105	135	150	500	145	95	70	4·27	115	95
6	1,530	1,500	80	130	140	150	750	130	105	70	4·58	110	130
										Meal			
7	1,250	1,110	25	90	110	125	750	55	110	55	4·40	145	150
8	1,475	1,110	20	85	15	45	1,000	200	150	140	9·49	85	100
9	1,475	1,170	50	105	110	35	(1,000)*	0	20	90	6·10	0	0
10	1,390	1,170	140	145	140	150	1,000	170	140	85	6·11	140	150
11	1,475	1,360	110	140	150	150	1,250	180	150	125	8·47	150	150
12	1,250	1,140	30	105	140	130	1,250	165	135	110	8·80	145	150

* Untouched.

with sawdust and did not drink much. By the third day all were well and normally active. The other six birds were treated in the same way but with an extract which had been prepared with water as the solvent. The exact toxicity of this extract was not known. The largest dose eaten by any bird corresponded to 12·75 g. per kilo, and the smallest to 6·08 g. per kilo. The birds showed the same symptoms as those given alcohol extract and were all fully recovered on the third day after the poison intake.

TABLE 20. *Toxicity of extract to chickens*

Bird No.	*Wt. (g.)*	*Takes of 1:2 rusk (g.)*					*Dose (g. extract per kilo body weight)*
		1	*2*	*3*	*4*	*5*	
						10% alcohol extract	
601	1,190	95	140	150	Starved	80	6·7
469	1,290	80	60	80		60	4·65
9	1,300	35	85	125		140	10·8
47	1,470	110	150	150		100	6·8
654	1,230	150	150	150		35	2·44
3	1,145	45	75	120		75	6·55
						10% water extract	
952	1,270	70	110	150	Starved	150	11·8
340	965	60	60	95		80	8·3
501	1,230	95	150	150		75	6·08
888	1,270	120	150	150		140	11·05
268	1,020	125	135	135		130	12·75
513	1,035	100	120	140		95	9·25

Evidence on other domestic birds is very scanty. Munch, Silver, and Horn (1929) fed powders at 10% in mash to pigeons but after a few pecks these refused to touch it. Doses of 2 g. per kilo injected into the crop produced no ill effect, but 3 and 4 g. per kilo produced emesis without other symptoms. Kamal and Hefny (1941) killed one out of two pigeons and two out of two ducks with an unstated dose of Egyptian squill powder in rat bait.

(*h*) Summary

1. Red squill, either as powder or extract, is a poison, of comparatively recent use, which is very popular, particularly in proprietary baits. This popularity may be based on the mistaken idea that it is harmless to man and domestic animals.

2. The reddish-brown powder is derived from the bulbs of squill (*U. maritima*), which occurs in most countries bordering on the Mediterranean. The bulb exists in different colour forms ranging from white to dark red. Various other species of the genus are also toxic but have not been used in rat control.

3. In preparing the powder the chopped-up bulbs may be dried in the sun, or in ovens, and the resultant squames ground to powder of various degrees of fineness. Much of the red squill of commerce is derived from the Algerian firm of S.A.L.A.M. and the process used by them is described.

4. The toxicity of the powder depends on a number of factors of which the most important are origin and colour of bulb, season of harvesting, method of preparation, and age of the sample. Red bulbs contain most of the rat-toxic substance and white bulbs the least, while all bulbs contain the heart-stimulant substance used in pharmacy. The amount of rat-toxic substance in the bulbs is greatest at the end of the flowering period in autumn and least in the spring. Some of the poison may be destroyed during the preparation of the powder, and the object of the S.A.L.A.M. process is to conserve as much of it as possible. The toxicity of powders may decrease with age, but a S.A.L.A.M. sample tested by the Bureau remained constant for two years.

5. The two sexes of rats show a very different sensitivity to red squill, the females being about twice as sensitive as the males. Samples of S.A.L.A.M. squill tested on a constant stock of male white rats showed L.D.s 50 ranging from 357 to 857 mg. per kilo, with one further sample that produced no deaths at 1,250 mg. per kilo. Tests on powders from other sources showed, in general, a much lower toxicity. It is stressed that the question of toxicity is very complex, and cannot be adequately summarized.

6. Extracts of red squill can be made by numerous processes and with numerous solvents. The extensive literature on this subject is discussed. Extractions at an experimental, and at a commercial, level were carried out with methyl alcohol as a solvent. Extracts made from powders of very low toxicity showed lethal doses which enabled them to be used successfully in the field.

7. The chemical nature of the rat-toxic principle is discussed. In pure form it appears to be a glycoside, called by its discoverers 'scilliroside', which has a L.D. 50 to male white rats of 0·70 mg. per kilo. The amounts found in the bulbs correspond closely with the toxicity of the most poisonous powders. The differential sex toxicity is found in the pure substance.

8. The symptoms of red squill poisoning are characterized by muscular spasms followed by a rolling movement, accompanied by diarrhoea. The onset of these unpleasant manifestations usually appears in a few hours, but death does not supervene for at least one day and may occur much later. Death from the extract is usually much more rapid, but the symptoms of illness are the same.

9. It is popularly supposed that both powder and extract are harmless to man and domestic animals. This fallacy is discussed and records of fatalities in several mammals including man are given. It is perhaps safer in use than other poisons because it is most unwillingly eaten, and because it gives rise to vomiting; but if these safeguards fail it is toxic.

There is good evidence that it is almost harmless to chickens, though even here deaths have been recorded.

5. ZINC PHOSPHIDE

(*a*) Introduction

Before the war zinc phosphide was a little-known rat poison in Great Britain. It had been used in ready-made baits by at least one servicing company since 1930, and was also available for retail sale in the same form; it was not, however, mentioned in the 1939 and 1941 editions of Bulletin No. 30 of the M.A.F., nor was it used much, if at all, in rat control by public authorities. Since 1942, when it was first recommended by the Bureau, it has become increasingly popular, and is now used perhaps more widely than any other poison. This popularity is due in part to its undoubted efficiency, and in part to the fact that rats killed with zinc phosphide tend to die on the surface so that operators can see the results. As shown in Chapter 4, the number of bodies is a very poor criterion of success; but nevertheless the psychological value is high.

Martelli (1919) records that zinc phosphide was first used by Grandi (1912) and Ghetti (1912) to combat plagues of field rodents in the Italian province of Modena in 1911. Martelli himself used it against field rodents in 1916 and carried out laboratory tests on efficiency of various concentrations in bait. From then onwards its popularity slowly increased and it was found efficient against rats by Tron (1931) and Turquois (1932) in extensive campaigns in Italian cities, and it continued to be extensively used against field-mice and mole crickets. By 1930 its use had been recorded in Germany (Schander and Götze, 1930), Great Britain (G. B. Patents, 1930, 333,216, and 333,218), and France (Aurivel, 1931), and a little later in the U.S.A. (Munch, Garlough, and Ward, 1936). In more recent work in these and other countries it has continued to prove an efficient poison, and Doty (1945) in Hawaii has compared it very favourably with thallium sulphate. In Germany aluminium phosphide has been used to evolve phosphine for deinfesting stacked foods, and it is suggested by Sassenhoff (1939) that this phosphide has been used in proprietary coated grain rodent poisons.

The only obvious disadvantage that it possesses is its danger to man and domestic animals from eating prepared baits and to man from inhalation of dust or evolved phosphine in the handling and preparation of baits. It has been stressed in the introduction that hazards of this type can be very largely eliminated by careful training, and should not be a deterrent from the use of an efficient poison by trained operators.

(*b*) Physical and chemical properties

Zinc phosphide (Zn_3P_2) is a greyish-black, dull powder, largely crystalline but containing some refractory particles. It is insoluble in water and alcohol and slightly soluble in alkalis and oils. It is fairly stable in air and water but readily breaks down in the presence of dilute acids, with evolution of phosphine (PH_3).

The Merck Index (1940) states that elemental phosphorus is slowly evolved by volatilization in dry storage, which accounts for the faint odour of phosphorus. When mixed in wet cereal baits, which are perhaps slightly acid, a much stronger odour of phosphine is found. This phosphine is apparently stable under normal conditions and no cases of spontaneous combustion have been reported. Commercial samples available in Great Britain have remained very constant in their character and no problems of comparison of toxicity or efficiency in the field have arisen. One manufacturing firm has stated that the phosphorus content of batches as first prepared varies from 24 to 22%. Zinc phosphide corresponding to 100% Zn_3P_2 would contain 74·9% zinc and 24·03% phosphorus, so that these batches vary from almost complete purity to 90% Zn_3P_2 content. There is evidence, however, that some samples contain a lower content. Thus Turquois (1932) states that Italian samples contained 18–20% phosphorus, i.e. 73–82% Zn_3P_2. The Merck Index (1940) states that commercial samples in the U.S.A. are said to be '80–85% pure'. A British sample examined by Dr. H. H. Green (personal communication) contained about 80% Zn_3P_2. The impurities are largely excess zinc, with a little phosphate. Elmore and Roth (1943) state that 'zinc phosphide manufactured by one dealer bears a guarantee on the label of "zinc phosphide 80%" but analysis indicated that this figure was a minimum and not the actual percentage of zinc phosphide present. Analysis of some of this material . . . gave total phosphorus 24·54 per cent and total zinc 74·21 per cent, corresponding to zinc phosphide 102·34 per cent and 97·90 per cent respectively.'

Because of its instability in the presence of dilute acids, it has been suggested that zinc phosphide might break down, with evolution of phosphine, when exposed in baits. It has also been suggested that such breakdown might be accelerated by the addition of weak organic acids in suitable amounts. If this could be arranged, it would be of great value as a safety factor, and would also eliminate the picking up of poison-bait residues. Elmore and Roth (1943) have examined the stability of grain baits, coated with poison, under laboratory and field conditions. Grain baits, coated with 0·56% zinc phosphide, were exposed to the weather on a roof; in 29 days they lost only 28·6% of the poison content. Under ordinary conditions of storage in burlap bags no deterioration was detected in 180 days. Two days' exposure to heavy rain reduced by one-third the content of a bait containing 0·21% poison, but 9 months' outdoor exposure of a bait containing 0·5% poison only reduced the content by 20%, when the bait was protected from rain. Doty (1945) has found a strong evolution of phosphine, with deterioration of the efficiency of the bait, when oat grains were coated with zinc phosphide in corn, linseed, and coco-nut oils. These oils, particularly if crude, turn rancid very quickly under Hawaiian conditions, but this difficulty can be eliminated by adding white neutral mineral oil in place of the vegetable oils, or by adding some substance to the vegetable oils which will neutralize any acidity which may develop. Doty has used mill lime (calcium hydroxide) which has proved very satisfactory at $\frac{1}{4}$–$\frac{1}{2}$% in linseed and corn oils, though with coco-nut oil even $\frac{1}{2}$% has not always been sufficient to stabilize the bait. Garlough and Spencer (1944) have mixed one part of magnesium carbonate with two parts of zinc phosphide as a poison for dusting on fruit, and Ward (in Doty) has stated that this has the effect of checking deterioration. Doty further adds that 'wet baits made with zinc phosphide will deteriorate rapidly, and soon become non-poisonous'. Some experiments conducted by Dr. H. H. Green (personal communication), to test this hypothesis, on 1:1 rusk baits containing 5% zinc phosphide, agreed in general with the findings of Elmore and Roth that deterioration under field conditions is slow and indicated that complete breakdown is not normally obtained.

Figures for particle size of two samples, dry sieved, are given in Table 21; the material not passing the 40-mesh sieve consists of a few large crystals or refractory particles and hard conglomerations of smaller particles; much of that which passes the 120-mesh sieve is very fine and tends to form a dust film on containers and apparatus used for bait mixing.

TABLE 21. *Particle sizes of zinc phosphide*

	Percentage by weight retained by sieve				
Sample	*40*	*60*	*80*	*120*	*> 120*
1	0·085	1·42	15·965	37·26	45·27
2	0·06	0·29	12·79	49·69	36·73

The toxic action depends on the breaking down of the zinc phosphide by the stomach acid with evolution of phosphine which is rapidly absorbed. A certain amount of the zinc is also absorbed but probably adds little or nothing to the toxicity. A method for the detection of zinc phosphide in small amounts has recently been evolved by Elmore and Roth (1943) and by Salt (1945). It consists of heating the material with dilute hydrochloric acid and absorbing the liberated phosphine in potassium permanganate. The phosphate thus formed is then converted into a reduced phosphomolybdic compound; the excess potassium permanganate is decolorized by the reducing agent. The colour developed, from greenish-blue to intense blue, is proportional to the amount of phosphate formed.

(*c*) **Toxicity and efficiency in the field**

The opinions of writers vary considerably as to what is the lethal dose of zinc phosphide. Two authors give very high figures, which must be based on either very different material or on insufficient tests. Schander and Götze (1930) give a minimum lethal dose (undefined) of 300–400 mg. per kilo; and Paranjothy (1939), for *R. rattus* of unstated weight, gives 45 mg. per rat, corresponding to about 300 mg. per kilo for a rat of 150 g. Garlough (1941) gives 25 mg. per kilo for *R. norvegicus*. No toxicity tests giving full details have been found in the literature. A preliminary and a full test have been carried out by the Bureau on male white rats, using the standard technique; these tests are summarized in Table 22 and, taken together, give a L.D. 50 of 41·3 mg. per kilo.

TABLE 22. *L.D. 50 of various other poisons*

Poison	*Tests*	*Sex of animals*	*Slope of regression line (b)*	*$\log_{10}$ L.D. 50*	*L.D. 50 (mg. per kilo)*	*Range (P = 0·95)*
Zinc phosphide . .	T. 81, 84	♂	6·611 ± 1·509	1·6164 ± 0·0283	41·3	35·9–47·7
α-naphthylthiourea .	T. 51, 52	♀	7·615 ± 1·976	0·6264 ± 0·0350	4·2	3·4–4·9
Barium carbonate .	T. 04, 05, 07	Old ♂	6·678 ± 1·630	2·8410 ± 0·0297	693	585–796
Sodium arsenite. .	T. 004, 005	♀	4·114 ± 0·785	1·6928 ± 0·0396	49·3	41·0–60·2

In the field zinc phosphide proved a most efficient poison after prebaiting, though not obviously superior to arsenious oxide (Ch. 4). Most experience was gained using a concentration of 5% with sugar-meal, soaked wheat, or rusk; but it was shown later that 2% was adequate with the latter. Soaked barley or maize was also used with success in the Middle East (Ch. 14). Stomach analysis proved that zinc phosphide in rusk was highly acceptable (Ch. 9); but results were erratic without prebaiting (Ch. 4).

(*d*) Symptoms and post-mortem findings

Rats poisoned in the laboratory show very characteristic symptoms, chiefly remarkable for the absence of any obvious signs of rapidly approaching death. With a strong dose the first symptoms appear within 20–25 minutes and death may occur in 70 minutes. With high doses death takes place within 24 hours and even with low doses usually occurs within 2–3 days; no cases have been recorded, as are characteristic of elemental phosphorus, of death being delayed for many days. In the field death is also usually rapid and bodies have been picked up within an hour of poison being laid. Rats which have taken a sublethal dose normally recover rapidly and show no persistent symptoms. Caged rats, after refusing further bait, lie down and show deep slow respiration. This state is interrupted by gradually decreasing attempts to break out of the cages. The death agony is very short, lasting only a few minutes, with a few strong convulsions. In the death posture, which has also been noted frequently in the field, the rat lies on its belly with legs and tail spread out. Further details of warning symptoms, and time of onset of illness and death, are given in Chapter 8.

Post-mortem findings are also characterized by the absence of any obvious cause of death. A number of white rats, some of which had received single lethal doses, others of which had received numerous sublethal doses, were examined for the Bureau by Dr. G. Roche Lynch and Professor W. D. Newcomb. Their findings may be summarized as follows:

All animals which received fatal doses died of heart failure with congested organs. Two of them had a little fat in the livers, in the periphery of the lobules; this was probably due to diet. Apart from this no abnormal fat was found in any of the animals. It is not clear why the hearts failed and death was too rapid for changes to be seen in the heart muscle, but it was not due to inability to obtain oxygen since the blood picture was normal. The rats which were sacrificed after repeated small doses showed no pathological deposition of fat. The organ most damaged was the kidney, where the first convoluted tubules bear the brunt of the poison, the cells showing necrosis, hyaline, and hypotrophic degenerations. The rest of the kidney was much less damaged and the glomeruli were nearly normal. The livers were only slightly affected. None had fatty changes but cloudy swelling, hyaline degeneration and necrosis were seen in the centre of the lobules. A little hyaline degeneration was seen in the myocardium.

In spite of statements to the contrary (British Pharmaceutical Codex, 1934) it is clear that the action of zinc phosphide is not like that of elemental phosphorus but like that of phosphine; it is indeed due to the evolution and absorption of phosphine in the stomach and intestines. Poisoning by phosphine is discussed in detail below.

In colonies of wild rats which have been poisoned with zinc phosphide baits it is usual to find many corpses relative to the number found with arsenious oxide (Ch. 4), though these are seldom more than half the numbers killed.

(*e*) **Danger**

(i) *Man*

The preparation and use of zinc phosphide as a rat poison presents three separate dangers to handlers: phosphine may be inhaled, particularly during the preparation of wet baits; fine dust rising into the air during the handling of the poison alone or in the preparation of dry baits may be inhaled; or baits may be accidentally eaten. Since zinc phosphide is a comparatively new rodent poison, very little information has been published on the dangers arising from its use as such, but there is a considerable body of work on the effects of phosphine on man and domestic animals. In these experiments, or cases, the phosphine is of course absorbed through the lungs and not through the stomach; but the symptoms and post-mortem findings are very similar to those found in cases where the absorption is through the stomach. They may also be used to show the very real danger to those mixing baits of evolved phosphine even in very dilute amounts.

The literature on the toxicity of phosphine has recently been summarized by Flury and Zernik (1931), Muller (1940), and Doty (1945). Flury and Zernik give a table, repeated by Klauer (1935) and by Doty, which gives the findings of various authors (Table 23). It will be seen that, even at very low concentrations, phosphine can produce serious symptoms. Corresponding figures for hydrogen cyanide in this table show that, although higher concentrations of phosphine are needed to produce rapid death, lower concentrations can produce serious symptoms after prolonged exposure, Muller had indicated that cumulative effects may sometimes be attained. These dangers can easily be avoided by simple precautions which prevent the building up of high concentrations of phosphine. Baits, particularly wet baits, should never be mixed in unventilated rooms; nor should workers remain for longer than necessary in rooms where such baits are being mixed. All containers both for the poison itself and for made-up baits should have tightly fitting lids; this is particularly necessary when baits are being carried in closed vehicles. Eight references containing cases of acute or fatal phosphine poisoning are given by Muller. Only a single case of poisoning from inhaled dust has been found in the literature: Elbel and Holsten (1936) report a case of an agricultural worker who became ill a few hours after handling zinc phosphide; he suffered severely for several days but recovered. It is probable that similar but less acute cases have passed unnoticed. Large quantities of the poison itself, or dry baits, should not be handled without the use of a dust mask. No cases have been found of voluntary or involuntary consumption by man of zinc phosphide in prepared baits, but it has never been suggested that man is in any way less sensitive to this poison than rats or domestic animals and birds.

TABLE 23. *Toxicity of phosphine and hydrogen cyanide to man (after Flury & Zernik, 1931)*

	Phosphine		*Hydrogen cyanide*	
	mg./l	*c/cbm*	*mg./l*	*c/cbm*
Rapidly fatal	2·8 or 2,000		0·3	270
Fatal in ½–1 hour or later	0·56–0·84 or 400–600		0·12–0·15	110–135
Very dangerous in ½–1 hour	0·4–0·6 or 290–430		0·12–0·15	110–135
½–1 hour without immediate or subsequent consequences	0·14–0·26 or 100–190		0·05–0·06	45–54
Effective after several hours (still some fatal cases after 6 hours)	0·01	7	0·02–0·04	18–36
Limit of effectiveness	0·002–0·004 or 1·4–2·8		..	..
Six hours without showing obvious symptoms	..	..	0·02(–0·04)	18(–36)

(ii) *Farm animals*

Vianello (1939) records the death of six out of nine horses, all of which were taken ill after nibbling residues of bait which had dried on to the edges of bait-mixing troughs. We ourselves have records of the death of four cows, unsubstantiated by post-mortem analysis, and that of a pig, in which 600 p.p.m. of zinc was found in the liver. Cats have frequently been killed during field operations, but all or most of these deaths have probably been caused by eating poisoned rats rather than the poison bait directly, a problem discussed below. Poultry and wild birds are all apparently very susceptible. Numerous cases of fowls being poisoned in the course of experimental field tests and control operations have been recorded by the Bureau. Hare and Orr (1945) and Blaxland and Gordon (1945) have recorded recent cases in this country, and Sassenhoff (1939) and Scheu (1939) have recorded deaths with proprietary poisons composed of grain coated with metallic phosphides, mostly zinc phosphide. Blaxland and Gordon (1945) have also carried out toxicity tests on poultry, giving the poison as a suspension in water by stomach tube. Seven chickens receiving from 400 mg. to 24 mg. per kilo all died; three receiving 26 mg., 15 mg., and 13 mg. per kilo are said to have shown no effects.

(iii) *Cats*

Evidence on the likelihood of cats and other carnivores being killed by secondary poisoning is conflicting, but it is clear that in some cases this can undoubtedly take place. Doty states that 'according to chemical theory, no secondary poisoning can occur when dogs or cats catch and eat rats which are sick, dying, or dead from zinc phosphide. This is true because the residual products of the phosphine gas (PH_3) produced in the rodent's intestinal tract are non-poisonous.' He has tested this assumption by feeding rats to cats and mongoose, the rats having been killed with zinc phosphide at 1% in oats. The amount of bait eaten is not stated. Three cats were fed one rat each, two of them at least eating the whole rat. No ill effects were shown, and presumably the cats did not even vomit. Two mongoose were fed rats over a period of 5 weeks, one eating five and the other seven. Mongoose also eat the whole rat including intestines, but no ill effects were shown.

Tests on six cats were carried out by the Bureau, the following method being used. Freshly dead rats which had consumed different doses of zinc phosphide were offered to cats in the laboratory. The rats were prebaited with unpoisoned sugar-meal or bread mash to ensure that they ate the whole bait, and then after 1 day's starvation, were given a weighed dose of zinc phosphide in the same base. As soon as the rats were dead they were opened ventrally and given to the cats, which had been prebaited the previous day with unpoisoned rats opened in the same day. It had been found that cats were reluctant to eat rats unless they had been opened. The results of these tests are given in Table 24. It will be seen that, although the cats vomit after eating rats containing zinc phosphide, they do not always do so in time to save themselves. Examination of stomach contents of rats killed in the field and in the laboratory has shown that amounts of zinc phosphide much in excess of 96 mg. may be consumed when offered at 5% in bait, up to 575 mg. per kilo body weight having been consumed in the laboratory and more in the field (Ch. 9).

TABLE 24. *Toxicity to cats of rats poisoned with zinc phosphide*

Sex	*Wt.* *(kg.)*	*Actual dose zinc phosphide (mg.) consumed via rat*	*Dose of zinc phosphide (mg. per kilo of cat)*	*Effect*
♀	2·0	192	96	Vomited, but dead next day
♀	2·0	144	72	Refused most of the rat
♀	2·2	96	44	Vomited, but dead next day
♂	2·6	96	37	Vomited and recovered
♂	2·6	96	37	,,
♀	2·2	72	33	,,

Doty's findings are at variance with those of the Bureau and may perhaps be accounted for by the low concentration in the bait. But if a rat ate a very full meal, e.g. 30 g. of bait containing 1% zinc phosphide, the amount actually eaten would be about 300 mg., a figure much higher than those found to be fatal in the Bureau tests. Doty's assumption that the zinc phosphide all breaks down via phosphine to give harmless residue is probably not valid, because in the stomach of an animal which has been poisoned some unbroken down zinc phosphide will be found even after some days, and a smell of phosphine will be present. Observations in the field have shown repeatedly that cats are killed by eating zinc phosphide poisoned rats. Two case histories may be given: (1) At a pig farm, three cats out of six were killed by eating rats poisoned with 5% zinc phosphide in sugar-meal. They were seen by the farmer's wife to be eating rats and two partly eaten rats were picked up later. (2) At a mixed farm a cat was seen eating a small rat the morning after poisoning with 5% zinc phosphide. It died 2–3 hours later.

(iv) *General*

As has been stressed by Schmidt (1936 *b*) it is probable that zinc phosphide is less dangerous as a secondary poison than substances such as thallium sulphate and strychnine, which remain undecomposed in the body for a long time and continue to be toxic in the tissues as well as in the intestine. Whether muscular tissues of animals poisoned with zinc phosphide are toxic has not been ascertained. A partial test was carried out by members of the Bureau with three geese which had been accidentally poisoned. One member suffered no ill effects from eating increasing amounts daily until a full meal was reached. Two other members who ate large quantities on the second day after the geese had died suffered from stomach pains and diarrhoea, but whether this was caused by the poison or by excess of goose was not ascertained. Rats fed with the uncooked flesh and liver showed no symptoms of poisoning.

There is apparently no fire danger attached to the use of zinc phosphide. The impure phosphine evolved from some of the less stable phosphides is spontaneously inflammable, but no cases have been recorded of the inflammability of that evolved from zinc phosphide.

(*f*) Antidotes

In cases of accidental poisoning, action must be taken rapidly because the poison is quick in action and must be removed or neutralized before it is broken down in the stomach. Garlough (1941) and Munch (1942 *a* and *b*) recommend the following treatment. Give an emetic of mustard; after vomiting has ceased, completely dissolve one 5-g. tablet of potassium permanganate in a glass of warm water and take; this oxidizes the phosphide to phosphate. Follow this 10 minutes later with half a teaspoonful of copper sulphate in a glass of water; this being intended to produce insoluble copper phosphide. Finally, give a purgative of one tablespoonful of epsom salts in water.

(*g*) Summary

1. Zinc phosphide is a poison, almost unknown in Great Britain before the war, which has increased greatly in popularity and is now used extensively by government departments and in proprietary baits.

2. It is a blackish powder which when dry smells slightly of phosphine. It is stable under normal conditions but breaks down in dilute acids and in some wet baits, thus gradually losing its toxicity. The samples examined have not given rise to any problems of purity or particle size differences. It is apparently never spontaneously inflammable.

3. The L.D. 50 to male white rats was found to be 41·3 mg. per kilo.

4. Rats poisoned in the laboratory are remarkable for the absence of symptoms of approaching death. Death may occur in less than an hour and even with low doses is usual in 2–3 days. A death posture, lying on the belly with legs and tail spread out is characteristic in the field and the laboratory. In the field it is usual to find a comparatively large number of corpses. Post-mortem findings are not like those of phosphorus.

5. Zinc phosphide is probably toxic to man and all domestic animals, and deaths of poultry have been frequently observed in field tests. Experiments on cats have shown that they may die from eating rats which have been poisoned with zinc phosphide, and similar results have been found in the field.

6. BARIUM CARBONATE

(*a*) Introduction

Barium carbonate has been for many years amongst the most popular of rodent poisons, but its use has declined sharply both in Great Britain and other countries during the war. The earliest records of its use that have been seen are those of Nessler (1861, in Schander and Meyer, 1923) and Crampe (1878), who used it to combat outbreaks of field mice in Germany, but it is probable that it was used in England at about the same time. Ableitner (1892) states that witherite, the naturally occurring barium carbonate, had long been known in England as one of the best mouse poisons, and an anonymous pamphlet published in London about 1870 gives a bait formula for rats containing this poison without any comment that it was new. Agricultural textbooks of the middle of the nineteenth century do not mention it, however.

Its popularity appears to have been based on the idea, at least partly fallacious, that it was relatively harmless to man and domestic animals. It is true that because it is less poisonous than most other substances used to kill rats there is less likelihood of accidents arising from consumption of the poison itself, but this advantage is much reduced in made-up baits because the percentage of poison used must be very high. The problem of danger is discussed more fully below but may be summarized in the words of Raebiger (1907) translated by Boelter (1909): 'I have come to the conclusion that preparations which are really non-poisonous are unable to kill rats, and poisons, call them what you like, if they kill rats will also kill domestic animals. In other words, there is no poison in existence which will only kill rats.' It is undoubted that barium carbonate will kill rats, but field tests by the Bureau have tended to show that it is the least efficient of the poisons generally employed. Other things being equal, the more toxic a substance the greater its efficiency, and barium carbonate is one of the least toxic. Seeds of doubt were sown by Crampe (1878) who found it the least suitable of the poisons then available in his campaign against field mice; and Grosbois (1904) reiterated this doubt. Its popularity in this country has however remained high since it was first officially recommended by the M.A.F. in 1920. It is now no longer mentioned in the seventh edition (1945) of Bulletin No. 30 of the M.A.F., but its use by official organizations is still considerable and it has been largely and often successfully used in the sewer campaigns organized under the auspices of the M.O.F. Our own field tests gave rather erratic results (Ch. 4). It is perhaps the commonest poison used in prepared baits for retail sale, this popularity being due to a continued belief in its relative harmlessness, to the fact that it is very cheap and that there are few legal restrictions. (Under Schedule 5 of the Poisons Rules (1935), made under the Pharmacy and Poisons Act (1933), retail prepared baits may be sold free from the restrictions imposed on substances contained in Part 1 of the Poisons List (1935).)

Barium chloride, usually as $BaCl_2,2H_2O$, has occasionally been tried as a rat poison, and, since it is in this form that barium is absorbed when rats are fed with the carbonate, it will be convenient to treat what is known of it here. Kunhardt and Chitre (1921) tried it as well as other barium salts, on black rats but found that it was most unwillingly taken. McDougall (1944) found the same with *R. conatus* in Queensland; he did kill a few rats but remarks that 'at all poison strengths with economic possibilities intakes are poor and irregular'. Obolenskii and Sokolov (1931) failed to poison brown rats with the chloride dissolved in beer.

(*b*) **Chemical and physical properties**

Barium carbonate, $BaCO_3$, is a white crystalline solid, almost totally insoluble in water (about one part in 50,000 of cold water), but acted on by dilute acids with the formation of the corresponding salt. In the case of hydrochloric acid, barium chloride is formed and it is this substance which is absorbed into the system when barium carbonate is taken by the mouth. It is stable in air and in baits. Commercial barium carbonate is prepared from two sources, from barium sulphate, which as barytes is a common mineral chiefly produced in Germany and the U.S.A., and from witherite, the naturally occurring carbonate. There are no economically important naturally occurring deposits of witherite except in England, the two most important mines responsible for the world production being in Northumberland and Durham. Commercial samples from both sources vary in colour from pale gray to pure white, a few being slightly tinged with yellow.

The following figures for particle size of two available grades of witherite are given by Muddiman (1942 *a*):

	1	*2*
Retained on 200 B.S. sieve	8–9	0·1
Through 200, on 240	5–6	0·2
Through 240, on 300	3–4	0·2
Through 300	81–82	99·5
	100·0	100·0
Oil absorption	9·0	12·0

(Precipitated $BaCO_3$ 21·0 g. linseed oil per 100 g. sample.)

The first sample is the one which has normally been used in rodent control in the past, the second being a recent product intended for use as an extender in paints (B.S.I. War Emergency Specification No. 926 of 1940). Barium carbonate prepared in various ways from the sulphate is usually very finely divided, almost all passing a 200 B.S. sieve, but detailed analyses are not available.

The importance of impurities in samples has been stressed by Melhuish (1920), Kunhardt and Chitre (1921), and Murphy (1922). These authors lay down a specification demanding 80% $BaCO_3$, not more than 8% of insoluble barium salts, chiefly sulphate and silicate and as free as possible from barium sulphide, chloride, oxide or hydroxide or sodium carbonate, which are stated to be repellent to rats. Two per cent. of these is the maximum allowed. The sulphate, being totally insoluble in the stomach acid, is unlikely to act as a deterrent, but the sulphide, giving off hydrogen sulphide in the presence of dilute acid, might well make baits unpalatable. All the samples examined in the Bureau, whether witherite or precipitated, have contained a much higher $BaCO_3$ content than 80%, and the possibly repellent impurities are correspondingly small. Witherite comes very pure from the mine and no further processing other than grinding is required; the standard whose particle size is given above (No. 1) shows the following as a typical analysis:

	%		%
$BaCO_3$	95–96	SiO_2	0·1–0·2
$BaSO_4$	3½–4	$CaCO_3$	0·05–0·1
FeS $FeCO_3$	0·05–0·1	Moisture	< 0·1
		Combustible matter	0·05
FeS_2	0·05–0·1		

A precipitated sample used in field work contained 98–98·5% $BaCO_3$ and 0·5–0·7% insoluble in hydrochloric acid, chiefly $BaSO_4$; barium sulphide never exceeds 0·2% and this is adsorbed. Very pure powders, corresponding to the 'Analar' standard of 99·89% $BaCO_3$ and no sulphide or sulphate, are available.

(*c*) **Toxicity**

(i) *Published information*

Schwartze (1920) was unable to trace any toxicity figures for the carbonate, though it had previously been realized that it was of fairly low toxicity since 25% had been recommended for use in baits. In his own tests on white and brown rats he does not work out a lethal dose but concludes 'the efficient lethal dose . . . [is] 630 mg. per kilo, or, more conservatively, 750 mg.'. The lowest dose which killed a white rat was 240 mg. per kilo, and none survived a higher dose than 775 mg. per kilo. Fifteen out of sixteen rats died after doses of 625 mg. per kilo. He defines the carbonate used as 'of high purity and free from soluble chloride, always used in the form of a fine powder, having been passed through a 100-mesh sieve'. This upper figure of 750 mg. has been frequently quoted since as the 'lethal dose' of the carbonate. Information on the black rat is available in Kunhardt and Chitre (1921) who used both a crude and a pure salt. On average sized rats (*c.* 90–100 g.) it was found that 97·5 mg. would kill 100% of the animals, representing a L.D. 100 of 975 mg. per kilo. In a more satisfactory test with the pure salt, in which the rats were divided up into weight groups, 97·5 mg. killed 100% of 20–80 g. rats, 89·3% of 100 g., 80·8% of 140 g., and 75% of 180 g. McDougall (1944) with Merck reagent quality carbonate has found the most conflicting results on *R. conatus*. This test is not easy to compare because the poison was not given at uniform strengths or in the same amounts of bait, but it was found that while one rat died at 309 mg. per kilo one survived the huge dose of 6·48 g. per kilo. Another rat survived two doses of carbonate of 3·02 g. and 5·23 g. and one dose of 1·57 g. of chloride in 12 days. A recent test on brown rats by Dieke and Richter (1946) gave a very high figure of 1480±340 mg. per kilo. Schwartze also carried out a toxicity test on the chloride and found that 4 out of 5 rats were killed at 355 mg. per kilo and 7 out of 7 at 533 mg. per kilo.

(ii) *Bioassays*

Bioassays were made of a standard example of witherite, the analysis of which is given above, during the months of April and May 1942, and since the results of these tests were somewhat perplexing, they are described here in some detail. The first of these tests (T. 04) was a preliminary one and was carried out using 4–5 animals at each dose level. These individuals were old male rats which had been used for breeding and which were to be discarded from the stock. The results suggested that the L.D. 50 was around 700 mg. per kilo, and on this basis it was decided to test twenty-five animals at a dose level of 600 mg. and another 25 at 800 mg. per kilo. Each group of twenty-five males was composed of two lots of animals, namely, old breeding males similar to those used in the preliminary test and, in addition, some young males around 3½ months old; most of them were probably just sexually mature, and they had not been mated. The differential mortality was as follows (T. 05):

Dose (mg. per kilo)	*Old*	*Young*
600	4/11 (36·4%)	5/14 (35·7%)
800	8/11 (72·7%)	1/14 (7·1%)

At the first of these doses there is evidently very little to choose between the two classes of animals, but at 800 mg. per kilo the difference is very marked. If this latter dose is considered by itself and a comparison is made between old and young, the value of χ^2 after applying Yates's correction for continuity is 8·8 and there is little doubt that this difference is significant ($P = <0·01$). The probits corresponding to the mortalities observed among the old rats fit in very well with the regression line given by the preliminary test, whereas those for the young rats are very irregular. It was then decided to check this result for young rats at 800 mg. and also to include a higher dose of 1,200 mg. per kilo. The young animals used in this test were approximately 3 months old. The mortality observed at 800 mg.

was five out of fourteen or 35·7% and that at 1,200 mg. five out of thirteen or 38·5%. These results on young individuals are again highly irregular and although the mortality at 800 mg. per kilo is higher than in the previous test, it does not achieve the degree experienced at this dose level by the old males in test T. 05.

Considering only the old males, the results obtained in the two tests (T. 04 and T. 05) were reasonably consistent and from the regression line based on these data it was estimated that a dose of 1,200 mg. per kilo should cause a mortality of between 90–100%. Accordingly a further group of thirteen old males was tested at this dose level (T. 07) and twelve of these died, a mortality of 92·3%. The animals in this last test were at least a year old. Although this test (T. 07) was not done on the same day as the previous test on young males (T. 06) there was only a 7 days' interval between them. If we compare the mortalities of old and young at 1,200 mg., $\chi^2_c = 6{\cdot}1$, a value which is high enough to suggest that there was some definite difference between these groups of animals.

The erratic results thus obtained in testing this sample of barium carbonate on young males around 3–3½ months of age are strikingly different from the much more consistent results of the tests made on old male rats. Unfortunately, the exact ages of the latter individuals were not recorded, but most of them were almost certainly over a year old. Some indication of the age differences between the two classes of animals is afforded by their comparative weight distributions, bearing in mind that we are here dealing with a homogeneous inbred strain of rats (Table 25).

TABLE 25. *Body weight distributions of rats used in toxicity tests of barium carbonate*

Body weight (g.)	*Old males*	*Young males*
140–	..	5
160–	..	12
180–	..	13
200–	2	13
220–	3	5
240–	9	5
260–	7	1
280–	10	1
300–	6	..
320–	10	..
340–	4	..
360–	6	..
380–	..	..
400–	1	..
Total .	58	55

We have no explanation to offer for the differences which have been recorded in this series of tests. Various possibilities suggest themselves, but clearly it would require a much more detailed investigation in order to see whether we are dealing with some chance phenomenon or whether there is in fact some physiological basis for this difference between old and young. Owing to a shortage of animals and the pressure of other work we were unable at this time to carry out any further tests with barium carbonate and thus the problem had to be left in this unfinished state.

The results from the tests made in 1942 on old males are, as we have already said, consistent and yield a reasonably good regression line. Basing the calculations on all tests on old males (T. 04, 05, 07) the L.D. 50 was estimated to be 693 mg. per kilo with a range ($P = 0{\cdot}95$) of 585–796 mg. This degree of toxicity is very similar to that recorded by Schwartze (1920) whereas the figures for young males much more nearly approach those of Dieke and Richter (1946).

Later, in May 1945, a number of fresh tests were made of the same sample of witherite, but this time females were used instead of males. The tests were in the nature of preliminary ones and a full series was not carried out. The results obtained are, however, of sufficient interest to be recorded. In the first two of these tests twenty old females and twenty young ones received doses of 500 mg. and over and all died. The L.D. 50 to females was clearly less than this figure, and the results of a third test suggest that it was somewhere in the region of 200 mg. per kilo. This degree of toxicity was very much higher than that recorded for males in 1942, though in this connexion it must be remembered that our rat stock in 1945 was possibly more sensitive than in 1942 if we may argue by analogy with the comparative results obtained in the cases of arsenic and squill. The difference between a L.D. 50 of nearly 700 mg. and one of round about 200 mg. per kilo seems, however, to be greater than could be accounted for by any change in the general level of susceptibility of our rat stock as a whole. Thus there may possibly be some sex difference in the toxicity of barium carbonate, though as far as we are aware no suggestion of this has been made by other workers. The evidence given here is, however, far from being conclusive and clearly a much more extensive series of comparative tests would be required to establish this point satisfactorily.

(*d*) Symptoms and post-mortem findings

Visible symptoms of barium carbonate poisonings show nothing very characteristic; they may occur within 2 hours if a large dose is taken or they may be delayed for as much as 48 hours. Time of onset is very variable even in rats treated with the same dose; this may be connected with the conversion of carbonate to chloride in the stomach depending on the acidity at any given time. Time of death also varies considerably, and again does not seem to be so closely correlated with size of dose as is the case with arsenious oxide. Out of a total of 111 fatal cases observed in the laboratory only 4 females died within 10 hours, but 44 out of 48 which had taken a dose of 750 mg. per kilo or more died within 2 days. These figures are in agreement with Pemberton (1925) who records death times from 'a few hours' to 8 or 9 days, but mostly within 3 days. Schwartze, however, found that 76% (53 out of 70) died within 16 hours and only 21% survived more than 24 hours. The chloride, as is to be expected, acts more quickly; thus Schwartze found that all 17 of his rats died within 16 hours. Field evidence also shows that death may take several days to occur. The only obvious symptoms are strong diarrhoea and diuresis; convulsions may sometimes occur in the early stages of illness but depression and paralysis set in before death supervenes. Wright (1936) states that death is due to depression of the medulla causing respiratory failure, but contributory factors are marked circulatory depression due to the toxic action on the heart and shock associated with superpurgation. It is often stated, e.g. Jack (1938), that rats poisoned with barium carbonate become very thirsty and therefore die away from their holes in search of water, and that the strong purging causes the corpses to dry up. This has also been said for other purging poisons but there seems little or no evidence that it is true. In colonies of wild rats which have been poisoned with barium carbonate it is not usual to find many corpses.

(*e*) Danger

Soluble barium salts, and those which can be broken down in the stomach, are all more or less toxic to mammals and birds, and the carbonate is no exception. It has been stated from time to time, e.g. Dewberry (1937), that it is much less toxic to the larger domestic animals, but little evidence has been found for this opinion. It is also stated that, when used as a rat poison in made-up baits, it is less dangerous than other poisons normally used; but the quantity taken in baits containing up to 25% barium carbonate will be almost as dangerous as the quantities of more toxic poisons used at lower percentages. Schwartze (1920) summarized the information available at that time and found no figures for toxicity of the carbonate to the larger domestic animals and has therefore drawn from the literature on the chloride. The carbonate, as has been explained above, is absorbed as the chloride, so that the toxicities can be compared, although such a comparison will not be exact, nor will the time of onset of symptoms and death be the same. Schwartze concludes 'the rat is the least susceptible of all mammals cited, although the absolute amount is less, because of the small size of the rat'.

In man, Kunhardt and Chitre (1921) quote a case of death after eating 3·9 g. and two of recovery after 35 g. and 29·5 g. For a man of 63·5 kilo (140 lb.) these figures would be 61·5, 550, and 465 mg. per kilo respectively. Stern (1896) records a death in 10 hours after 8·6 g. of the chloride, corresponding to 130 mg. per kilo. No chronic effects have been noticed from frequent handling of the powder: Mr. E. W. Muddiman (personal communication, 1940) has recorded a man working in dry dust for years, during which time he ground over 1,000 tons, without any ill effects.

A considerable body of information is available on its toxicity to cats and it is clear that the cat is more sensitive than the rat. This sensitivity is increased by the fact that the carbonate does not produce emesis as readily as some other poisons, particularly arsenious oxide; Kunhardt and Chitre found that their experimental animals frequently vomited, but it has been the experience of the Bureau, both in the laboratory and in the field, that relatively few animals do so. These authors, experimenting with twenty-one cats, record the following deaths, and note that all survivors were made ill: 1/6 at 195 mg.; 3/6 at 325 mg.; 4/6 at 650 mg. and 2/4 at 975 mg. The irregularity of these results is explained as being due to the vomiting, so that the real amount retained is not known. Deaths occurred in 5–31 hours. The weights of the cats are not given but they are said to have been adults. If the average weight is taken as 2·5 kg. it will be seen that cats may have died from as little as 78 mg. per kilo, though others survived 350 mg. per kilo. Boulenger (1920) and White (1919) state, without detail, that cats can withstand 648–972 mg.

Since the danger to cats in the field lies more in their eating sick or dead rats than from eating the poison baits, an experiment was undertaken in the Bureau to find out roughly how toxic poisoned rats would be. Tests on nine cats, two being used twice, are summarized in Table 26. The poison was given to laboratory white rats in 1-g. bread paste baits; usually the rats were allowed to die but sometimes they were killed when they became ill; the thorax and abdomen were opened ventrally to make them more acceptable to the cats. The rats were eaten readily though not always completely, and vomiting only occurred in two cats, a small amount in one case and about 17% weight of the rat in the other. It will be seen that, while some cats survived eating rats containing 202–607 mg., three died from eating rats that contained 418, 640, and 660 mg. of poison. While it is not possible to say how much poison remained to kill these cats, an upper limit on the lethal dose is obtained from the assumption, clearly false, that none was lost. The three fatal doses then correspond to 190, 200, and 245 mg. per kilo body weight of cat.

Dogs are much less sensitive. Kunhardt and Chitre (1921) failed to kill thirteen dogs at doses ranging from 650 mg. to 9·1 g.; but no body weights are given. Laboratory dogs vary greatly in weight, but for dogs of 10 kg. these doses would correspond to 65–910 mg. per kilo. White gives an M.L.D. of 9·1 g. without details. Boulenger states that an average sized dog survived 6·48 g. No toxicity figures for horses, cattle, sheep, or pigs are known but Dieckerhoff (1895) gives figures for the chloride: horses survived 8–12 g.; cattle survived 40 g.; sheep survived 4 g. but succumbed to 6 g. Reynolds (1907) killed a horse at a dose of 15 g. per 1,000 lb., corresponding to 33 mg. per kilo. Muddiman (personal communication, 1940) has recorded the death of eight pit ponies from eating corn accidentally contaminated with the carbonate. Kunhardt and Chitre tested the resistance of chickens and found the following fatalities: 0/5 at 195 mg.; 0/5 at 325 mg.; 5/10 at 650 mg., 7/10 at 975 mg.; 9/9 at 1,300 mg.; 8/8 at 1,625 mg.; and 13/13 at 1,950 mg. For chickens of 2 kg. the L.D. 50 would therefore be about 325 mg. per kilo. Boulenger (1920), however, records that chickens can withstand 648–972 mg.

TABLE 26. *Toxicity to cats of rats poisoned with barium carbonate*

Cat. No. and sex	*Wt. (kg.)*	*Wt. of rat (g.)*	*Amount of poison given (mg.)*	*Parts of rat uneaten and % of total wt.*	*Amount of rat vomited*	*Effect*
1 ♂	3·00	270	202	Legs, head, and tail	None	None; ate well after
12 ♂	2·20	250	418	Half head, some skin and tail. 12%	,,	Dead 3 days later after refusing food
15 ♂	2·20	210	440	Hind leg and skin. 4%	Some very liquid material	None; ate well after
11 ♂	2·45	295	441	Half head, some skin. 6%	None	None; ate well after
13♀	2·48	195	496	Skin, head, and part of stomach. 33%	,,	None. As such a large proportion of the rat (including a part of the stomach) remained uneaten this test may not be conclusive
15 ♂	2·20	115	506	Small piece of skin. 3%	,,	None; ate well after
1 ♂	3·00	150	510	None	,,	Ill with typical symptoms of barium carbonate poisoning
14 ♀	2·60	240	520	Small piece of skin. 2%	,,	None; ate well after
16 ♂	2·89	205	607	Head, front, and rear legs, lumbar muscles, genitalia. 31%	Mass of undigested material. 17%	None. As half the rat remained uneaten or was vomited this test is not conclusive
5 ♂	3·20	200	640	Hind legs; some liver	None	Dead next day
4 ♀	2·70	220	660	Head, skin, legs, tail	,,	Dead next day

(*f*) Antidotes

The antidotal treatment recommended consists of the administration of an emetic, mustard being the usual, followed by magnesium sulphate, which Munch (1942) states is 'to counteract the barium action, to relieve the stimulation, and to form the relatively insoluble barium sulfate'. Excess of magnesium sulphate has a laxative action and helps to expel the barium sulphate.

(*g*) Summary

1. Barium carbonate was first used in the nineteenth century and became very popular, particularly in proprietary baits; this popularity was probably due to its high lethal dose. It is now much less used by government departments and is no longer officially recommended.

2. It is a white solid, insoluble in water, which is found as a mineral in a very pure state in Great Britain. Commercial samples are often derived

from the sulphate. Analyses for particle size and impurities are given; the presence of traces of sulphides may affect acceptance in the field.

3. In toxicity tests with mineral barium carbonate the L.D. 50 for old male white rats was 693 mg. per kilo; but there was evidence that young males were more resistant.

4. Barium carbonate is toxic to man and to domestic animals and birds. In field tests there has been some accidental mortality amongst cats, and in laboratory experiments vomiting has not usually been produced. The relatively low toxicity acts as a safeguard, but this is offset by the high percentage needed in a bait.

7. OTHER POISONS

(*a*) **Phosphorus**

Yellow phosphorus was first used as a rat poison in Germany and was introduced into this country by Carr (1842), from which time its use has greatly increased and it has retained a constant popularity. However, it has been very largely confined to commercial preparations, because of the difficulties of mixing a poison which is spontaneously inflammable in air and insoluble in water, as well as being dangerous to handle and liable to give rise to serious chronic illness. These difficulties can only be overcome by manufacturing organizations preparing baits in bulk; phosphorus is an impractical poison for those who make up small quantities of bait at a time. Also on account of these physical characteristics very little efficient laboratory or field work has been published about it. It is, however, an efficient poison and is used in a commercial preparation which probably has the largest sale in this country.

As prepared commercially, phosphorus is a yellow solid, minutely crystalline with a waxy lustre; it is usually cast in sticks. Insoluble in water, it is slightly soluble in ethyl alcohol (1 in 350 parts of 90% alcohol), more soluble in ether (1 in 80), chloroform (1 in 25), olive oil (1 in 80), and very soluble in carbon disulphide (2 in 1). It melts at 44° C. It is unstable in air, under certain conditions is spontaneously inflammable, and is therefore kept under water. Various oxides form on exposure to air; if the heat is in any way conserved such as by wrapping loosely in paper, by covering with various catalysts or exposure in a finely divided state, phosphorus will burst into flame. At normal pressure and temperature above 7° C. it gives a luminous glow attended by a slight rise in temperature; white fumes and an unpleasant garlic-like smell are emitted, probably indicative of the presence of phosphine. The red allotrope which is usually present in commercial phosphorus in small quantities is non-toxic and not spontaneously inflammable.

No detailed tests of the toxicity to rats are known; the usual figure given is 100 mg. per kilo; Beeley (1939) indirectly indicates a lethal dose similar to that of arsenious oxide which he takes as 125 mg. per kilo; Dewberry (1937) however gives a very much lower figure than this as a minimum (6 mg.) and a higher as a maximum (200 mg.). The problem of finding an accurate lethal dose would be complicated by the instability, the particle size, and individual physiological response. In cases of human poisoning it has been shown (Reveil, 1859) that if phosphorus is taken in large lumps it may pass through the gut without producing any poisoning symptoms at all. It is most toxic in a finely divided state, as when deposited by carbon disulphide evaporation. In rats there is no evidence for individual differences in response, but in man, even when the phosphorus is known to be in a finely divided state, widely differing doses for fatal and non-fatal poisoning are found. Lobel (in Blyth and Blyth, 1920) mentions a case of a lunatic who died from 7·5 mg. and Taylor (1948) one of a child that died from 8 mg., though recovery has taken place from nearly 50 times this amount.

Unlike all other rat poisons, phosphorus cannot be mixed dry with simple cereal baits such as those advocated by the Bureau; some method must be used to distribute it finely and evenly throughout the bait and to prevent it burning spontaneously. Two methods have been recommended. It may be mixed with sugar, molasses, water, oil, or fat and heated until it melts, the other ingredients then being stirred in. Secondly, a solution in carbon bisulphide may be mixed with the rest of the ingredients, the carbon bisulphide being allowed to evaporate. Provided that the unpleasant smell of the solvent can be eliminated, this method is probably the best, because it avoids heating the mixture, and impurities are mainly insoluble and settle out. When sugar or oils are used the particles of poison become coated, thus reducing the risk of fire: the usual percentage used is between 2 and 4%, the latter being possibly too high for safety. Resulting baits are of course pastes and typical formulae and instructions for their composition are given by Beeley (1939), Dewberry (1937), and Schlupp (1920).

The symptoms of phosphorus poisoning are well known in man, but for rats only the external and more obvious symptoms are recorded. Onset of illness may be very quick or may be delayed for some hours, depending greatly on the condition of the poison in the bait; where the particles are occluded in oil or sugar, onset may be delayed. Time of death also varies greatly but is seldom less than 2 days or more than 7. Occasional cases are recorded in man and domestic animals in which death only occurs after several months. Intense diarrhoea is the most characteristic symptom, resulting in desiccation which is said to reduce the chance of corpse smell. Fatty degeneration of the liver is found at post-mortem even where death has been very quick.

Much has been written about the dangers of the use of phosphorus both as a rat poison and in industry, and it is certainly true that it has been the cause of more poisoning cases than any other rat poison; but it must be remembered that, with the possible exception of arsenious oxide, it is used by the general public more than any other rat poison. Further it is the poison of the smallholder and the householder, while arsenious oxide is the poison of the farmer and gamekeeper; the former are not accustomed to handling chemicals in the normal routine of life. Phosphorus is a very popular method of suicide; Blyth and Blyth (1920) state that it comes fourteenth on the list of suicide poisons, though many of the cases are not due to rat poison. In their words 'it is an agent, which, before the change of manufacture, was within the reach of anyone who had two sous in his pocket wherewith to buy a box of matches; but to the educated and those who know the horrible and prolonged torture ensuing from a toxic dose of phosphorus, such a means of exit from life will never be favoured'. The unpleasant garlic-like smell should be a considerable safeguard against accidental poisoning of man, but in made-up baits this is very small. Though the smell is very marked in a freshly opened tin of proprietary rat poison, it decreases considerably on exposure to air.

Where phosphorus is handled frequently in the preparation of baits, very careful precautions must be taken to avoid contamination which may result in serious chronic illness. The well-known necrotic condition known as phossy-jaw, at one time a serious industrial hazard in the match industry, is the result of continued contamination with minute amounts. The further danger of fire has been discussed.

(*b*) Thallium sulphate

Thallium sulphate is a comparatively new rodent poison. The element itself was only discovered in 1861 by Crookes but both it and its salts were soon known to be toxic to man; it is recorded by Munch and Silver (1931), however, that the sulphate was not used in rodent control until about 1920 when it was marketed in Germany in a proprietary product. Introduced into the U.S.A. in 1924 it gained a rapid popularity both against rats and field rodents. This popularity has continued since and led to its use, though on a more limited scale, in many other countries including Great Britain. During the war years its use has declined somewhat because of shortage of supplies.

Both its advantages and disadvantages lie in the readiness with which it is accepted in baits—through the apparent absence of any taste to indicate its presence. According to the consensus of opinion of workers in the U.S.A. it is more willingly accepted than any of the other poisons, particularly by field rodents, of which some species are notoriously shy of accepting poison baits; it also gives more consistently successful results. This degree of acceptability, however, extends to other mammals and birds, and campaigns in the open against field rodents have resulted in a spate of literature on its real or imaginary danger to wild life. Other disadvantages are found in its solubility and its cumulative and chain effects; these are discussed in detail below. An advantage is its very slow action, though the absence of an efficient antidote minimizes this. In Great Britain, at least, its greatest disadvantage is its high cost, far greater than that of any other poison used, and for this reason it is never likely to become popular. The literature on thallium sulphate and other poisonous thallium salts is very extensive, but much of it has been summarized by Munch and Silver (1931) to which paper the reader is referred for more detailed information.

Thallous sulphate, Tl_2SO_4, is a white crystalline substance stable in air and in baits, soluble in water to the extent of 36·8 g. per 100 c.c. water at 10° C. The chemically pure substance is apparently tasteless and odourless but Munch (1937) has noted that some commercial samples are not accepted as willingly as others, particularly by prairie dogs (*Cynomys* spp.), and one sample had a definitely unpleasant smell. The L.D. 50 has usually been given as 25 mg. per kilo for white rats; this figure is based on a test carried out by Munch and Silver (1931) which gave eight out of eleven deaths, or 73%, at this dose; 20 mg. per kilo gave three deaths out of eight, or 37%. A further test on five rats in each group gave 60% mortality at 20 mg. per kilo and 100% at 25 mg. per kilo. The figures for these tests are also given in Munch (1928). Dieke and Richter (1946) have given a L.D. 50 of $15{\cdot}8 \pm 0{\cdot}9$ mg. per kilo for wild *R. norvegicus*, the thallium sulphate being administered in 10% acacia solution; these authors also quote an unpublished figure of 31 mg. per kilo for white rats found by the U.S. Fish and Wildlife Service.

Provided that a suitable percentage of poison is used in a suitable base, all published evidence indicates that thallium sulphate is a very successful poison in the field. The most extensive field data on its use are those of Dopmeyer (1937), Barnum (1930), and Doty (1945) in Hawaii. After extensive use of barium carbonate and strychnine it was found that thallium sulphate gave the most satisfactory kills. The baits used at first were coated wheat, with or without oil, made up into 'torpedoes', containing poison at 0·1%. These were used direct, but later the bait was changed to loose rolled oats, prebaiting was used, and the percentage of poison increased to 0·4%.

Relative to the other poisons, thallium sulphate is very slow acting. No detailed evidence is available on time of onset of illness, but it is probably fairly late; with large doses death may occur in 36 hours but, with just fatal doses, may be delayed for as long as 6 days (Dieke and Richter, 1946; Schern, 1930; Munch and Silver, 1931). Munch and Silver give the following summary of symptoms and post-mortem findings: 'Loss of appetite usually occurred [after 24 hours] followed by respiratory distress, labored breathing and death by respiratory failure. Post-mortem, the heart was engorged; the lungs and liver usually congested, and in some cases the kidneys and spleen; the stomach lining was hemorrhagic.' Paralysis of the hind limbs is a frequent and characteristic symptom.

Apart from the usual dangers inherent in the handling and field use of any poison, thallium sulphate possesses certain characteristics which increase the hazards. It has a cumulative effect so that the repeated absorption of small sublethal doses may give rise to serious illness. Teleky (1928) records that only three out of fourteen workers in a firm making proprietary rat baits did not show symptoms of chronic thallium poisoning. Sublethal doses either single or cumulative may cause prolonged illness of which alopecia is the most striking symptom; Doty has recorded that when the percentage of poison was low hairless rats were caught. Its solubility allows absorption through skin lesions, and rubber gloves should be used when mixing baits. A chain effect has also been recorded; Garlough (1935) has stated that the possibilities of secondary poisoning in the field are remote, but Doty has noticed a great reduction of mongoose populations in treated areas, and has observed cats and mongoose showing symptoms of thallium poisoning. Numerous cases of accidental poisoning have been recorded in man and domestic and wild animals. Munch (1937) records 788 cases of human poisoning with forty-six deaths; the majority of these cases were caused through medicinal uses, particularly as a depilatory; but twenty-one cases with five deaths were from rodent baits. Amongst others, Linsdale (1931) gives a long list of birds and mammals killed in field rodent campaigns in California. It has been suggested that extensive use by broadcast methods might damage plant growth and give rise to semi-permanent soil sterility, but Horn *et al.* (1936) have shown that only amounts greatly in excess of those normally used could cause such damage.

Munch (1937) has formulated an antidotal regimen for thallium poisoning but stresses that it is based on tests on animals: (*a*) intravenous administration of sodium iodide until thallium has practically disappeared from the 24-hour urine; (*b*) sodium thiosulphate intravenously; (*c*) pilocarpine and calcium salts intravenously; and (*d*) oral administration of hydrochloric acid.

(*c*) **Alpha-naphthylthiourea**

Alpha-naphthylthiourea, 1-(1-naphthyl)-2-thiourea, known as Antu, $C_{10}H_7NHCSNH_2$, is a war-produced rodenticide, recommended by Dr. C. P. Richter (1945) as a result of researches on the toxicity of a long series of thioureas and related compounds undertaken at Johns Hopkins University, Baltimore. It was used extensively during the latter years of the war and is now available in proprietary baits on the American market. In this country it has been used at an experimental level, has recently been recommended by the M.O.F. (1946), and is available in at least one proprietary bait.

Antu is a greyish white fine powder, insoluble in water, stable in air and in baits. Like other thioureas it has a bitter taste only discernible by some people, but this taste is much less strong than that of the soluble phenylthiourea. Its toxicity to adult wild *R. norvegicus* is given by Dieke and

Richter (1946 *b*) as a L.D. 50 of 6·9 mg. per kilo; these authors also quote an unpublished L.D. 50 found by the U.S. Fish and Wildlife Service of 8 mg. per kilo. Tests carried out by the Bureau on female white rats gave a L.D. 50 of 4·2 mg. per kilo (Table 22). A detailed analysis of age variation in resistance has been made by Dieke and Richter (1946 *a*) who show that suckling rats are much more resistant, giving a L.D. 50 of 58±4·4 mg. per kilo. They were unable to show any sex variation in resistance. *R. rattus* is much more resistant (Dieke and Richter, 1946 *a*). Richter (1945) states that doses between 100 and 400 mg. per kilo are required and the U.S. National Research Council (1946) and Rieman (1946) give a figure of 250 mg. per kilo or higher. The house mouse is more resistant than the brown rat but the lethal dose is below 100 mg. per kilo.

Antu is much slower in action than sodium fluoroacetate, the other highly toxic substance recommended in America. Dieke and Richter (1946 *b*) give a survival time for wild brown rats of 16–30 hours and Upton (1945) states that black rats may be found dead in campaigns up to 8 days after laying the poison. The most constant and obvious symptom is a strong pulmonary oedema and pleural effusion, resulting in death from drowning.

The most extensive field tests that have been reported so far are those of the group of workers in Baltimore. A high kill is said to have been obtained over large areas of the city by using direct poisoning with various baits at 2–3% concentrations and also by dusting the powder on to the surface of water or directly into burrows and runways. Details of baiting methods are given by Richter and Emlen (1946). A series of tests carried out by the Bureau gave excellent results after prebaiting; but with direct poisoning failures were as frequent as with other poisons (Ch. 4).

A tolerance can be built up by sublethal doses, and it has been advised not to use this poison on the same site at intervals of less than a month. Shyness has also been recorded which may cause caged rats to refuse baits containing Antu for as long as 6 months. Its use against black rats is not recommended; whether its inefficiency against this species is due to the lower toxicity or to unpalatability has not been made clear. Upton found very unsatisfactory results in Texas, and Murray (1946) in Pacific islands states that it 'will kill rats but is generally no more effective than red squill'.

Antu appears to be building up for itself a reputation for harmlessness to man and some domestic animals. Rieman (1946) states 'A field agent of the Fish & Wildlife Service says, "Antu appears to be relatively less toxic to other animals than anything except red squill. It is next to squill in safety on the list of poisons".' A recent number of *Pests* (1946, Vol. 14, No. 7) contains two advertisements which state 'it is harmless to poultry' and 'comparatively safe to humans'. The published facts may be summarized as follows. The only evidence for man is that no cases of illness or death have been recorded. Twelve monkeys (*Macaca mulatta*) given poison by stomach tube in 10% acacia solution gave a L.D. 50 of 4,250 mg. per kilo, but Dieke and Richter (1946 *b*) state that part of this tolerance may be due to emesis which was fairly frequent in the test animals. Intraperitoneal injection gave a figure of 175 mg. per kilo. Adult chickens also gave a L.D. 50 of 4,250 mg. per kilo and even by injection one of 2,500 mg. per kilo; but Anderson and Richter (1946) found that 3- to 5-week chicks died when fed 2% or 3% Antu in mash. Rieman also records death of chickens. Dogs are relatively sensitive, giving a L.D. 50 of 56±4 mg. per kilo. Cats are more resistant (500 mg. per kilo) but both cats and dogs have been killed in the field. Pigs, according to Anderson and Richter (1946), ate mash containing 2% poison freely and died from as little as 37·5 mg. per kilo. It is clear therefore that, with the possible exception of its effect on adult chickens, Antu must still be regarded as a dangerous poison to man and domestic animals.

(*d*) Sodium fluoroacetate

Sodium fluoroacetate, also called by its test substance number '1080', is a new rodenticide, FCH_2COONa, recommended by the U.S. Fish and Wildlife Service as a result of testing more than 1,000 substances which might have been suitable. The first announcement of its properties was made by Kalmbach (1945) since when a considerable amount of information has been published on its efficiency in the field. It has been used at an experimental level in this country. As has been pointed out by Klingensmith (1945), Marais (1943, 1944) has found that monofluoroacetic acid is the toxic principle of *Dichapetalum* (*Chailletia*) *cymosum* (Hook) Engl. called gifblaar, a well-known stock poison in South Africa. The seeds of a related species, Magbevi nut (*D. toxicaria* Don), are used by West African natives for murder and as a rat poison (Renner, 1904; Power and Tutin, 1906; Rabinovitch, 1928), being known as ratsbane or broke-back (Lindley, 1846). Methyl fluoroacetate and various other organic fluoroacetates have also been examined for toxicity (McCombie and Saunders, 1946; Chenoweth and Gilman, 1946).

Sodium fluoroacetate is a white, flaky, crystalline substance, soluble in water and with a characteristic smell. Murray (1946) states that it is highly hygroscopic under tropical conditions. Its toxicity to *R. norvegicus* is recorded by Dieke and Richter (1946) as 0·22 mg. per kilo±0·01 mg., the same authors record a figure of 3 mg. per kilo found by the U.S. Fish and Wildlife Service. Kalmbach (1945) records 5 mg. per kilo for brown rats, 2·5 mg. per kilo for white, and 0·1 mg. per kilo for *R. rattus*.

Dieke and Richter record death in 45–240 minutes, but it is probable that in the field death may occur much more quickly; Murray found that bodies could frequently be picked up close to bait containers, indicating that illness came on so rapidly that the rats were unable to move farther away (but see Ch. 4, § 3 (*c*)). Nothing has yet been published on the symptoms and post-mortem findings, but Chenoweth and Gilman (1946) working with the methyl salt found that the symptoms in the rat were not like those of other animals tested, except the hamster. There is a latent period of 1–2 hours after which the animals are tremorous and excitable; convulsions of myotonic type can be produced by stimulation. The rats tended to remain huddled together with heads curled under. Death usually took place in 4–6 hours, apparently due entirely to respiratory failure. Corpse smell is mentioned by Macchiavello (1946) as being of frequent occurrence.

In the U.S.A. this poison has been used extensively, particularly in the southern states; it has also been used in Peru by Macchiavello and in various Pacific islands by the U.S. Navy. Baiting with 0·1% in water solution is said to give good results, though cereal and oil-containing baits have also been used. Murray notes that it gives a spectacular kill but that cage tests had indicated a repellent factor which was confirmed in the field. Its hygroscopic property makes it difficult to use in dry baits in the tropics.

It is apparently almost equally toxic to all other animals tested. Kalmbach gives L.D. 50's ranging from 5 to 0·1 mg. per kilo for various rodents, 0·35 mg. per kilo for dogs, and 10 mg. per kilo for chickens. Macchiavello notes that fleas died after feeding on poisoned rats. On account of this very great toxicity it has been recommended that it should be used only by trained people (Monsanto Chemical Company, 1946). Dykstra (1950) reports that there have been twenty-two human casualties, mostly in children under five years of age. Twelve of these cases were fatal.

(*e*) Sodium arsenite

Sodium arsenite should perhaps be treated merely as an appendage to arsenious oxide, and much that has been said about that substance, particularly its physiological action and symptomatology, applies equally well. In other ways, however, it is markedly different in its suitability as a rat poison, these differences depending largely on the fact that it is soluble in water. Its toxicity is higher, its dangers greater, and its acceptability probably less. Any solution of arsenious oxide in alkaline water may be considered as an arsenite solution, but control by solid sodium arsenite is comparatively recent, probably being derived from its use against insect pests, particularly locusts, and as a dip for farm animals. Most of the recommendations for its use have come from tropical countries, and it is uncommon or unknown in this country. Schlupp (1920) and Kunhardt and Chitre (1921) mention it, the former as a replacement for arsenious oxide in wet baits and the latter as one among several substances tested for acceptability, and seldom if ever used in the field.

Commercial sodium arsenite is a white amorphous powder; crystalline forms have been prepared in small quantities, but the commercial substances are not of constant composition. Hance (1938) says that the exact chemical composition of a solution of arsenious oxide in sodium hydroxide is not known, but that compounds having the formula $NaAsO_2$, Na_2HAsO_3,

$Na_2H_4As_4O_9$, and others may be present. Commercial samples are easily soluble in water; the solubility will vary from sample to sample, probably within small limits—that of the sample used in the toxicity tests given below being about 45 g. in 100 c.c. at normal temperature. The filtered solution is straw yellow. The amount of impurity in this sample was less than 1%, the following being the composition by weight:

	%		%
As_2O_3	80–81	NaCl	0·3
Na_2O	15·3–16	Fe_2O_3	0·06
H_2O	2·8–3·4	Insoluble . . .	0·1

The only figure for toxicity that has been found is that given by Kunhardt and Chitre (1931) of an M.L.D. for young adult *R. rattus* of 0·33 g. A test carried out by the Bureau gave a L.D. 50 of 49·3 (41·0–60·2) mg. per kilo for female white rats (Table 22).

Sodium arsenite is odourless but has a sharp salty taste. The physiological action is like that of arsenious oxide, but the onset of symptoms is quicker and time to death shorter, presumably because of its solubility.

Several authors such as Paranjothy (1939), Beeley (1939), South (1931), and Schlupp (1921) give bait formulae for use in tropical countries but there is almost no evidence on efficiency in the field. Kunhardt and Chitre (1921) working with caged *R. rattus* found that sodium arsenite was less acceptable than barium carbonate in a ratio of 32·8 to 100, both being offered in bajri dough (spiked millet *Pennisetum typhoideum*). In a small test carried out in the Bureau on caged *R. norvegicus*, the take of poison bait was very small. The poison was offered in 20 g. wholemeal flour, with no starvation period, the rats having previously been fed on whole wheat (Table 27).

TABLE 27. *Takes of sodium arsenite in wholemeal flour by white rats*

Sex	*Weight (g.)*	*% sodium arsenite*	*Amount of bait taken in 17 hours (g.)*	*Result*
♀	150	3	0·00	nil
♀	140	4	0·35	dead
♀	160	5	0·15	nil
♂	375	6	0·90	,,
♂	345	7	0·65	dead

Numerous fatalities to domestic animals have been reported (Du Toit and Steyn, 1936; Thornton, 1938), particularly when this poison has been used as a spray in locust campaigns. No figures are known for the lethal dose to man but fatal cases have been reported from South Africa, and numerous cases of inflammation and burns caused by contact between the poison and the bare skin have also occurred (Gaud, 1937; Thornton, 1936).

(*f*) **Strychnine**

The sale of strychnine, except for medicinal and research purposes, is forbidden in this country by Rule 15 of the Poisons Rules (1935) made under the Pharmacy and Poisons Act (1933). Two exceptions have been made to this rule and it is possible to sell strychnine, with certain restrictions, to kill moles and seals. Its use has therefore died out; but it has a long history as a rodent poison and is still used extensively in countries where sale is not restricted; the more important characteristics may therefore be briefly summarized. In the form of the ground-up seed of *nux vomica* or St. Ignatius' bean (*Strychnos nux-vomica* L.) it was used as a mammal poison at least as early as Parkinson (1640) and gradually became popular; Smith (1768), whose work is clearly based on personal experience, praised it against arsenious oxide. It continued in popular use and was the poison ingredient in a number of proprietary rat baits until 1935.

Nux-vomica contains a number of alkaloids of which the most important are strychnine and brucine; the latter is much less toxic but is used in one proprietary bait as a substitute for strychnine. The alkaloid itself, white crystalline and only very slightly soluble in water, and the sulphate, which is fairly soluble, are the forms in which it has most frequently been used. The nitrate and hydrochloride have sometimes been tried. Both the alkaloid and the sulphate have a bitter taste though the former is said to be less strong.

Strychnine and its salts are very highly toxic to all mammals. Dieke and Richter (1946) have given a L.D. 50 of 4·8±0·4 mg. per kilo for wild *R. norvegicus* and quote a figure found by the U.S. Fish and Wildlife Service of 6 mg. per kilo. An earlier figure is that of Schwartze (1922) who found that white rats were relatively resistant, needing 25·6–32 mg. of sulphate per kilo to produce 100% mortality. Munch, Ward, and Garlough (1937) showed that commercial samples of the alkaloid and sulphates prepared from them showed a wide range of toxicity, the alkaloids giving L.D. 100's ranging from more than 25 to 7·5 mg. per kilo.

The characteristic symptoms of strychnine poisoning, violent muscular spasms, appear very quickly after ingestion, often in a few minutes; and death due to paralysis of the central nervous system generally results in ½ hour, though it may be as rapid as 12 minutes.

Although it has been used very extensively in the past, more recent opinion suggests that it is not very efficient against rats. Garlough (1941) states that it 'is not recommended, because the rats detect its bitterness too quickly and only a very small per cent of them will eat a sufficient quantity of the bait to kill them. Strychnine is, however, used in bait for house mice.' Doty (1945) also concludes that it was not efficient in the field and that some rats showed a comparative immunity.

The combination of very high toxicity and very rapid and violent death is the reason why the sale of strychnine in this country is restricted. A further danger lies in its great stability which can give rise to secondary poisoning of animals eating the corpses. Apparently it is not so toxic to birds, particularly game-birds, as it is to mammals, and in consequence its use to combat field rodents has not been attended by such casualties as occur when thallium sulphate is used (Heinekamp, 1925; Gabrielson, 1932).

(*g*) Summary

1. *Phosphorus*. The use of elemental phosphorus is limited by the difficulties of preparation: it is thus almost entirely confined to proprietary baits. In these however it is very popular, no doubt deservedly. Detailed figures for toxicity are unavailable, and will vary greatly with the particle size and the nature of the bait base. Onset of symptoms may be rapid but death may be very slow. Phosphorus is dangerous to handle because it is cumulative in its effects; there is also a danger of fire if it is used at too high a percentage in the bait.

2. *Thallium sulphate*. Thallium sulphate has been used extensively in the U.S.A. and Germany and to a lesser extent in Great Britain. It is apparently very successful, being taken readily by many rodent species, but its use is limited by its extremely high cost. The most recent toxicity figure for brown rats gives a L.D. 50 of 15·8 mg. per kilo. It is a slow acting poison, the most rapid deaths occurring in 36 hours, but 6 days being not unusual. Repeated small doses may have a cumulative effect so that it must be handled with great care; it may also act as a chain poison. Its tastelessness to man is an added danger.

3. *Alpha-naphthylthiourea*. This substance was produced as a result of war-time research in the U.S.A. and has now been used in Great Britain. The L.D. 50 to brown rats is 6·9 mg. per kilo, but the black rat is much more resistant, giving a figure of several hundred mg. per kilo. A strong pulmonary oedema and pleural effusion is characteristic. A surprisingly great range of toxicity to domestic animals has been recorded; chickens are highly resistant, but the possibility of danger to man is not yet disproved.

4. *Sodium fluoroacetate*. This is another poison that has been recommended in the U.S.A. as a result of war-time research. It is characterized by its very high toxicity, 0·22 mg. per kilo for brown rats, and the very short illness, death occurring in 45 minutes in the laboratory and perhaps more quickly in the field. Dogs, chickens, and other rodent species are apparently all very sensitive to this dangerous substance. Twenty-two human casualties had been reported by 1950.

5. *Sodium arsenite*. Sodium arsenites of indefinite chemical constitution have occasionally been used as solid poisons; and solutions of arsenious oxide in dilute sodium alkali should also be regarded as arsenites. A commercial sample of known composition gave a L.D. 50 of 49 mg. per kilo for female white rats. There is evidence that this poison is often refused in

the field. Its other characters are similar to those of arsenious oxide except that there is the added danger to man of absorption through the skin.

6. *Strychnine*. Strychnine as a rat poison has been illegal in Great Britain since 1935; but it still has extensive use in other countries. Both the alkaloid and its salts, the sulphate being that most commonly used, have a L.D. 50 of a few mg. per kilo. The most characteristic symptoms are violent tetanic spasms, and death usually takes place very rapidly; the only safeguard is the extremely bitter taste. It is not broken down in the body and may therefore act as a chain poison.

APPENDIX 1

THE TOXICITY OF ARSENIOUS OXIDE TO WILD BROWN RATS

By H. V. THOMPSON *and* P. H. LESLIE

THE Bureau's inbred stock of Wistar strain white rats is presumed to have had a very homogeneous genetic composition. The average resistance of the rats to a specific poison varied very little from generation to generation, thus permitting accurate comparisons between different poisons and different samples of the same poison. The importance of using the same inbred laboratory stock for these comparisons must be stressed, since we have found that the estimated L.D. 50 of the same sample of squill was much lower for the white rat stock of Imperial Chemical (Pharmaceuticals) Ltd. at Manchester, than for our own stock. Also Munch *et al.* (1937) found great variation in the L.D. 50 of the same sample of red squill tested in three different laboratories. Wild brown rats from the same or different localities would be expected to possess a heterogeneous genetic composition and, consequently, different samples would be expected to show considerable variation in their average resistance to a specific poison.

Since it is the wild rat that we ultimately wish to poison, it was thought valuable to do a series of toxicity tests on wild brown rats paralleling those on white rats, in order to test the susceptibility to our standard poisons of different wild rat populations and to compare this susceptibility with that of the Bureau's inbred stock of white rats. Refined Canadian arsenious oxide (sample 31) was tested. For white female rats the L.D. 50 was 121 mg. per kilo with a range ($P = 0{\cdot}95$) of 113–30 mg. per kilo, or 93–107% (Table 1, p. 45).

The wild rats used in these tests were caught either in live traps at various localities, or by hand at threshings. Freshly caught rats were usually kept for a few weeks in outdoor colony cages (Ch. 4) and fed upon wheat. (T. 2 was an exception when rats were taken into the laboratory immediately.) They were then captured, weighed, and sexed, and put in individual 12-in. square × 6 in. high laboratory rat cages. After being fed upon wheat for 1 or 2 weeks, they were reweighed and fed upon a surplus of flour and water paste for 1 day. This was followed by 24 hours' starvation, and they were then offered a poison dose made up in a 1-g. pellet of flour and water paste.

Two major difficulties are to be met with in making bioassays upon wild animals. Firstly, it is difficult to obtain animals that are of uniform sex and weight, and, secondly, they are much more laborious to handle. The first difficulty was to some extent overcome by deciding to use no animals of less than 100 g. in weight and to apply the different dosages in a random order as regards sex.

Six wild brown rats, tested in February 1943, all died after consuming a dose of 150 mg. per kilo of arsenic, the bait-base being sausage-rusk with twice its weight of water. This indicated the possible upper limit for poison doses in subsequent tests.

Four tests were done in 1944 upon rats from Gloucestershire, Wiltshire, Oxfordshire, and Northumberland, seventy-four rats being used in all. The bai base in all cases was flour and water paste (the standard base for white rat toxicity tests). Doses ranged from 40–100 mg. per kilo body weight of rat (Table 28). In three of these tests the approximate value of the L.D. 50 can be estimated. Thus, in the case of T.2, if we assume that at a dose of 25 mg. per kilo 0/8 would have died, the L.D. 50 is 46 mg. per kilo. For T.4 and T.5 the approximate L.D. 50's are 80 and 76 mg. per kilo respectively. Evidently, from the scale of these three tests, these approximate estimates would be subject to large errors. In order to test whether the general level of susceptibility to this sample of arsenic was similar, or differed significantly between these populations, a single regression line was fitted to the 9 points and its goodness of fit tested by means of χ^2. The second approximation to the regression line gave:

TABLE 28. *Refined Canadian arsenic tested on wild brown rats (mixed sexes) (weight range 100–425 g.)*

Dose	*T.2*	*T.3*		*T.4*	*T.5*
(mg. per kilo)	*Glos.*	*Wilts.*	*Oxford*	*Northumb.*	*Oxford*
40	..	..	0/11	..	..
50	5/8	..	..	2/8	0/8
60	..	1/7	..	..	..
75	..	..	..	..	3/8
100	8/8	..	..	5/8	6/6

$$y = 4{\cdot}8191 + 6{\cdot}111\,(x - 1{\cdot}8178),$$

where y is the probit for the value of $x = \log_{10}$ dose. The value of χ^2 was 20·98 and for 7 degrees of freedom $P < 0{\cdot}01$. This high value of χ^2 suggests strongly that there were marked differences between these populations.

The L.D. 50 given by the regression line is 70 mg. per kilo. Since, in the present instance, there is no particular reason for abandoning the hypothesis of a linear relationship between the probits and log dose, the usual expressions for variances of y and b were multiplied by $\chi^2/n = \dfrac{20{\cdot}98}{7} = 3$. In calculating the range by the exact method, the value of t was taken $= 2{\cdot}365$, corresponding to the $P = 0{\cdot}05$ level for 7 degrees of freedom, and the range was estimated to be $(P = 0{\cdot}95)$ from 49–135 mg. per kilo, or 70–192%.

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APPENDIX 2

THE FATE OF RESIDUAL RAT POISONS IN CORN-RICKS

By R. B. FREEMAN

Poisoning of rats and mice in corn-ricks is often carried out by the same methods as used in farm buildings and rural infestations generally. These methods involve the use of poisons which are toxic to man, domestic animals, and poultry; and many farmers and pests officers have hesitated to place poison baits in corn-ricks for fear that the grain or other products of threshing may become contaminated with the poison.

To test this supposition poison baits containing zinc phosphide or arsenic have been placed in ricks and an analysis made of the products of threshing. The following baits which have been recommended by the Bureau, and are in general use, were tested:

		% by weight
1.	Wholemeal flour	75
	Sugar	10
	Arsenious oxide	15
2.	Sausage-rusk	45
	Water	45
	Arsenious oxide	10
3.	Wholemeal flour	85
	Sugar	10
	Zinc phosphide	5
4.	Sausage-rusk	47½
	Water	47½
	Zinc phosphide	5

To ensure that the results should be comparable with or worse than the most dangerous conditions that could apply in practice the following precautions were taken in all ricks. Ricks were chosen which had very few or no rats or mice in them. Each rick received an amount of poison bait which contained 60 g. arsenic or 30 g. zinc phosphide—corresponding to 400, 600, 600, and 600 g. bait of the four formulae given above. This amount is higher than the amount that would normally be used in poisoning an infested rick. The baits were all placed in artificially made holes between 4 and 6 ft. from the ground, whereas in practice they would be scattered in real rat holes from the ridge to the base.

Products and method of sampling. Except in the case of the whole grain satisfactory methods of sampling could not be devised because the bulk of the products is very large compared with the size of the samples taken. A considerable lack of uniformity within the samples is to be expected on theoretical grounds, and therefore no attempt was made to produce uniformity by grinding or other methods before analysis. It will be seen that, in the three samples tested for uniformity, considerable range was found. Terms used for the various products of a threshing operation vary; the following terms have been used here:

(*a*) *Whole grain.* Dressed grain was sampled by taking a few grammes from each of the first ten sacks filled after the 6-ft. level had been reached. Examination of the grain in sacks taken during the threshing of the poisoned part, when sausage-rusk baits had been used, showed no particles of bait.

(*b*) *Tailings.* Some threshing machines separate only one grade of tailings consisting of chipped and small grains, the very small and broken grains coming out with the weed-seed and dirt; others separate the tailings proper, the second tailings or broken grain, and weed-seeds into three. Only about one sack or less of tailings comes through during the threshing of the poisoned part and samples were taken of a few grammes each, every time a sack of whole grain was filled; i.e. ten small samples. Tailings are used as animal and poultry feed and, under war economy, to a small extent for human consumption. Examination of tailings and broken grain when sausage-rusk baits had been used showed that small particles of bait were clearly visible. In the case of arsenic baits the particles of bait were blue owing to the presence of colouring matter; and in the case of zinc phosphide baits they were grey.

(*c*) *Broken grain.* Where this fraction was present only a few pounds came through during the threshing of the poisoned part, and the samples were taken from a few random handfuls. In ricks which are heavily infested with mice and to a less extent in ricks which are heavily infested with rats, the amount of tailings and broken grain increases considerably. Broken grain is used as poultry feed.

(*d*) *Weed seeds.* This fraction may be separated at the rear of the machine in a sack or underneath the machine beside the chaff or in both places. In ricks from fields heavily infested with weeds the seed content may be high, but from clean land it consists largely of earth and dried and broken leaves. Only a few pounds came through during the threshing of the poisoned part, and the samples were taken from a few random handfuls. When the seed content is high this fraction may be sold to mills for cleaning or to pet shops for bird seed: when the seed content is low it is considered as waste but may be picked by poultry.

(*e*) *Chaff.* Because of its great bulk relative to its weight the chaff was difficult to sample. Samples were taken from a few random handfuls from the fraction coming through during the threshing of the poisoned part. Chaff is used in animal feed.

(*f*) *Straw and cavings.* No samples were taken.

(*g*) *Dust.* During threshing the air surrounding the machine is more or less heavily laden with dust, varying with atmosphere conditions, particularly wind, and with the condition and content of the rick. After initial experiments with a hand-operated dust pump, it was found impossible to collect dust samples sufficiently large for analysis. However, a considerable accumulation of dust and larger airborne particles collects on the ledges of the machine. Samples were therefore collected from these ledges. They were dusted down just before the 6-ft. level was reached and the samples taken when the 4-ft. level was reached.

All the ricks were wheat, except one (No. 4) which was barley. Although no measurements were taken of their size or their content of the various products, they were all of typical south of England ricks; oblong, with perpendicular or sloped ends, about 20 × 16 × 18 ft. high to the ridge.

The detailed findings are given in Table 29 and may be discussed as follows.

Sugar-meal baits. These baits remained dry and tended to blow away when the sheaves were pitched on to the platform of the threshing tackle, nevertheless much of the bait went into the drum. In general, though the number of samples is not large enough for statistical analysis, more poison tends to appear in the dust and less in the weed-seeds, broken grain, and tailings when sugar-meal is used.

Sausage-rusk baits. Although these baits dry out to some extent they are still damp when passed through the drum. The larger particles and conglomerations

TABLE 29. *Poison residue in threshing products*

(*p.p.m.* As_2O_3; *p.p.m.* '*Zinc phosphide*')

	Arsenious oxide controls				*Arsenious oxide 15% in sugar-meal*					*Arsenious oxide 10% in sausage-rusk*				*Zinc phosphide 5% in sausage-rusk*			*Zinc phosphide 5% in sugar-meal*			
Rick number:	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*
Dust < 100	3	3	2	Tr.	200	80	130	5	105	160	10	6	20	Tr.	10	Tr.	Tr.	Tr.	6	6
Dust > 100					50	40				30										
Seed	Tr.	Tr.	3	20	20	7	40	280	80	..	150	380	480	80/40	80	150	5	140	110	150
Chaff	Tr.	Tr.	Tr.	Tr.	..	..	12	3	Tr.	..	6	Tr.	Tr.	4	10	6	4	..	8	4
Broken grain	Tr.	Tr.	Tr.	Tr.	..	..	Tr.	..	..	700	53	Tr.	Tr.	..	..	150	..	75	90	130
Tailings	Tr.	Tr.	Tr.	Tr.	6	8	Tr.	Tr.	Tr.	..	48	Tr.	45	Tr.	Tr.	9/Tr.	Tr.	90/40	Tr.	Tr.
Whole grain	Tr.	Tr.	Tr.	Tr.	..	..	Tr.	Tr.	Tr.	6	Tr.	Tr.	Tr.	Tr.	Tr.	Tr.	Tr.	Tr.	Tr.	Tr.

Tr. = < 1 p.p.m. for arsenious oxide and < 2 p.p.m. for zinc phosphide.

of the smaller particles are treated by the riddles as broken grains or weed-seeds. More poison tends to appear in the weed-seeds, broken grain, and tailings and less in the dust.

Whole grain. Except for No. 4, the barley sample, no estimable amount of either poison was found.

Tailings. In most samples no poison, or very little appears in the tailings; but in three cases amounts ranging from 90 to 40 p.p.m. came through.

Broken grain. Where this fraction was present considerable amounts of poison was usually found, ranging from 700 p.p.m., the highest figure recorded, to three samples with only a trace.

Weed-seeds. High figures were also recorded for this fraction, all samples contain some poison ranging from 480 to 486 p.p.m. It may be noted that one of the control samples showed 20 p.p.m. arsenic for weed-seeds. No explanation can be offered for this, and it seems improbable that any plant product would contain as much arsenic as this naturally.

Chaff. Although poison appeared in many of the chaff samples the amount was always small, ranging from 12 to a trace p.p.m.

Dust. The amount of poison appearing in the dust was very variable but was sometimes high.

It is clear from these figures that considerable amounts of poison do appear in the products of threshing when rat baits are laid under the conditions described. Whilst there is no legal limit to the amount of arsenic in food intended for human consumption, there is a tradition that the upper limits should be 1·4 p.p.m. for solids and 0·14 for liquids. This tradition is based on the findings of the Royal Commission on Arsenical Poisoning (1901) and the figures are absurdly low. The findings of various authors show that natural animal foods may contain very much higher quantities than this, although this arsenic is probably never present as arsenious oxide.

Zinc phosphide does not of course occur in nature and legal limits have never been considered; it is nevertheless a much more highly toxic substance than arsenious oxide and recent work and field experience has shown that it is particularly toxic to poultry.

Figures expressed in parts per million give, perhaps, an exaggerated idea of the real amount of poison present. In the case of a chicken eating broken grain containing the maximum figure found of 700 p.p.m., it will be seen that a lethal dose cannot be consumed. A pullet of 1,400 g. will eat about 100 g. dry food a day. If the entire meal was of broken grain, which is very improbable, only 70 mg. arsenious oxide would be eaten, corresponding to 50 mg. per kilo. If the average arsenic content in tailings for the samples of both baits is taken, i.e. 150 p.p.m., only 10 mg. per kilo would be consumed. It must also be remembered that the poison was put in the ricks under conditions very much more severe than would occur in practice.

Nevertheless, it is clearly possible that domestic animals and poultry might be poisoned by eating broken grain, tailings, or seed. Chaff may be considered quite safe and whole grain is safe by being entirely free from poison.

The analyses were carried out at the Veterinary Laboratory of the M.A.F., Weybridge. I am exceedingly grateful to Dr. H. H. Green for his ready co-operation.

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3

CONTAINERS FOR BAITING

By CHARLES ELTON *and* R. M. RANSON[1]

I. INTRODUCTION

THIS paper gives an account of two kinds of bait containers for field use in experiments and in poison campaigns against rats, that were developed and tested by the Bureau of Animal Population during the war, and found to have advantages for a good many purposes. The two types are (1) the P3, a specially shaped box in which the bait is inaccessible to animals other than rodents, (2) the blocked drain-pipe, easier to make, but less specific to rodents. Such containers have been used very widely in experimental field tests, for measuring prebait, poison bait, and postbait takes by rats; and they have also found a use, though more limited than was at first anticipated, in practical control campaigns.

(*a*) **Containers for semi-permanent poison baits**

Containers were originally intended for semi-permanent poison baits, and a note on this subject is inserted here to complete the research records. Rodent control strategy depends upon a successful balance between efficiency of method, supplies of materials, supply and cost of labour, and the degree of safety to species other than rodents. In 1938 Elton had the idea that poison baiting might be organized according to epidemiological principles by the placing of a network of semi-permanent poison bait points. Some ideas on the subject were put forward at the Sixth Pacific Science Congress in 1939. They were worked out without special reference to rat control, rather against the background of the sylvatic plague problem in western United States and the vole problem in British forest plantations.

> At present, poisoning campaigns are very costly and are responsible for a great deal of ecological disturbance which may even in some instances defeat the object of control. I consider that the control of rodents by poison should be conducted according to epidemiological principles, and that this could be carried out by the adoption of semipermanent poison baits of a type only accessible to rodents, and distributed in such a density that the rodent in question would automatically be killed off in sufficient numbers to maintain approximately the desired density. Experiments along these lines are being started in Oxford. They are made possible by the existence of census methods with which to plan and test operations. It appears to me that the purely technical aspects of such research should present no more difficulties than many much more difficult achievements in applied

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[1] R. M. Ranson died in September 1944. This chapter incorporates the substance of reports by him on this work, in which he took a leading part.

science. The method would perhaps make it possible to apply planned moderate control over very wide areas at a low cost, and to sterilize completely areas where there is exceptionally serious danger from rodents. By a system of this kind it would be possible to iron out fluctuations to some extent, to retain moderate numbers without complete extermination (which is undesirable from many points of view), and apply rational control in the light of definite knowledge of the habits and population dynamics of the various species. (Elton, 1939.)

In 1938–9 Ranson began some preliminary research on semi-permanent poison points for use against voles (*Microtus agrestis*). Before these had gone very far, the war switched his attention to rat control, and considerable research was done by him, from which he developed the P3. There were three main aspects to be studied: (1) poison baits that would keep for a considerable time without losing palatability, going mouldy, &c.; (2) a container specific to rats, for placing the baits in the field; (3) field tests, with census methods, for measuring the efficiency of the semi-permanent baiting system. It should be explained at once that this investigation was begun before the feeding habits of the rat had been studied extensively and the significance of prebaiting had been understood. The reasons for failure or comparative inefficiency of the direct poison baiting methods so widely used against the rat are fully discussed in Chapters 4 and 5, and it is now realized that the system of semi-permanent poison baits originally envisaged has little chance of working efficiently against the brown rat, at any rate with the range of poison and baits commonly used in this country. For the rat at any rate, it is also realized that bait points have to be placed at the tactically correct points for a given infestation, so that any permanent system of poison points would inevitably tend after a time to lose contact with some of the rat population. There is also evidence that baits in containers do not always draw the whole of a rat population into them. The failure in these earlier experiments of semi-permanent container baits (mostly cereal grains such as wheat, treated or coated in various ways with poisons, with or without attractant substances as well) was undoubtedly due to the same set of factors that militates against all types of direct poison baiting (see, however, Chapter 4, § 19).

A parallel set of experiments was undertaken by Hewer at Slough in the search for a semi-permanent poison bait for use in sewers. An appropriate bait carrier was designed. The poison baits evolved were successfully semi-permanent even in exacting conditions equivalent to those of a warm and damp sewer, but the enterprise was abandoned at this stage partly because severe bombing of the London area at that time made it impossible to organize underground field tests, and partly owing to the development of the prebaiting methods which later proved so successful in sewers and elsewhere.

The result of research was therefore to prove that there was no hope of saving labour by the use of 'Permanent Poison Points'—this having been one of the chief immediate war-time motives for the investigations. But it was found that the type of container that Ranson had invented, the P3, could be very usefully employed both in experimental and in some control work, since it gave protection against disturbance by other animals, also to some extent by man, and from weather, and was convenient for making accurate measurement of bait takes by rats. P3 was therefore adjusted to mean 'Protected Poison Point', and an account of its principles and structure is given in the next section.

An additional reason for giving attention to rodent-specific containers was that the only rat poisons generally available during this period of the war (1940–2) and the only ones in large supply in 1943 were inorganic ones dangerous also to other animals. This situation was caused by the using up of stocks of the comparatively rat-specific organic poison red squill, and the closure by the enemy of the Mediterranean squill supply area; and also by the recognition of the possible dangers and some other disadvantages of using rat 'Virus' (*Salmonella* bacterial cultures), formerly believed to be also specific to rodents (Leslie, 1942).

It will be seen that the idea of semi-permanent poison baiting systems, employing containers, broke down mainly because the poison baits of the type commonly used in Britain, when offered to rats without preliminary prebaiting do not cause sufficiently enthusiastic consumption to ensure a high percentage of lethal doses being taken by the rat population. It is realized that it may be possible to produce baits which are sufficiently toxic or attractive or slow-acting to ensure efficient kills, and that these might be adapted for semi-permanent baiting. During the war and post-war periods the shortage of various 'attractive' foodstuffs has made this line of approach an unrealistic one, at any rate in Britain.

2. THE PROTECTED POISON POINT, OR P3

(*a*) General design

The photographs in Plates 1 and 3 show what P3 looks like when placed ready for baiting. It is essentially a wooden box with a tunnel cut through the lower part of one side and some subsidiary devices for increasing its specificity and for preventing accidental spillage of bait. Fig. 1 gives a general idea of the structure. The essential thing that makes P3 specific (in Britain at any rate) to rats (and other small rodents) is that a rat entering the tunnel has to turn through three planes in space to get into the inner bait compartment. The line of reasoning which led to this invention by Ranson may be of interest to record here.

P3 has three component parts: the tunnel, the 'go-up', and the baffle.

The tunnel. A number of shapes and designs of container, mostly consisting of tunnels, were made and tested on wild rat populations; but it was found to be very difficult to get rats to enter these, and it was thought that the reason was because the rats disliked setting foot on a floor which was strange to them, whereas they would readily go through tunnels with no floor. For a time very tall tunnels, 8 in. high, were used, as both Boulenger (1920) and Hovell (1924) insist that the rats go through tunnels of these dimensions much more readily than they do smaller ones (Ch. 5). In searching for new rat colonies one feature of habitat choice was noticed; wherever a shed is put up near a rat infestation, rats very soon come to live

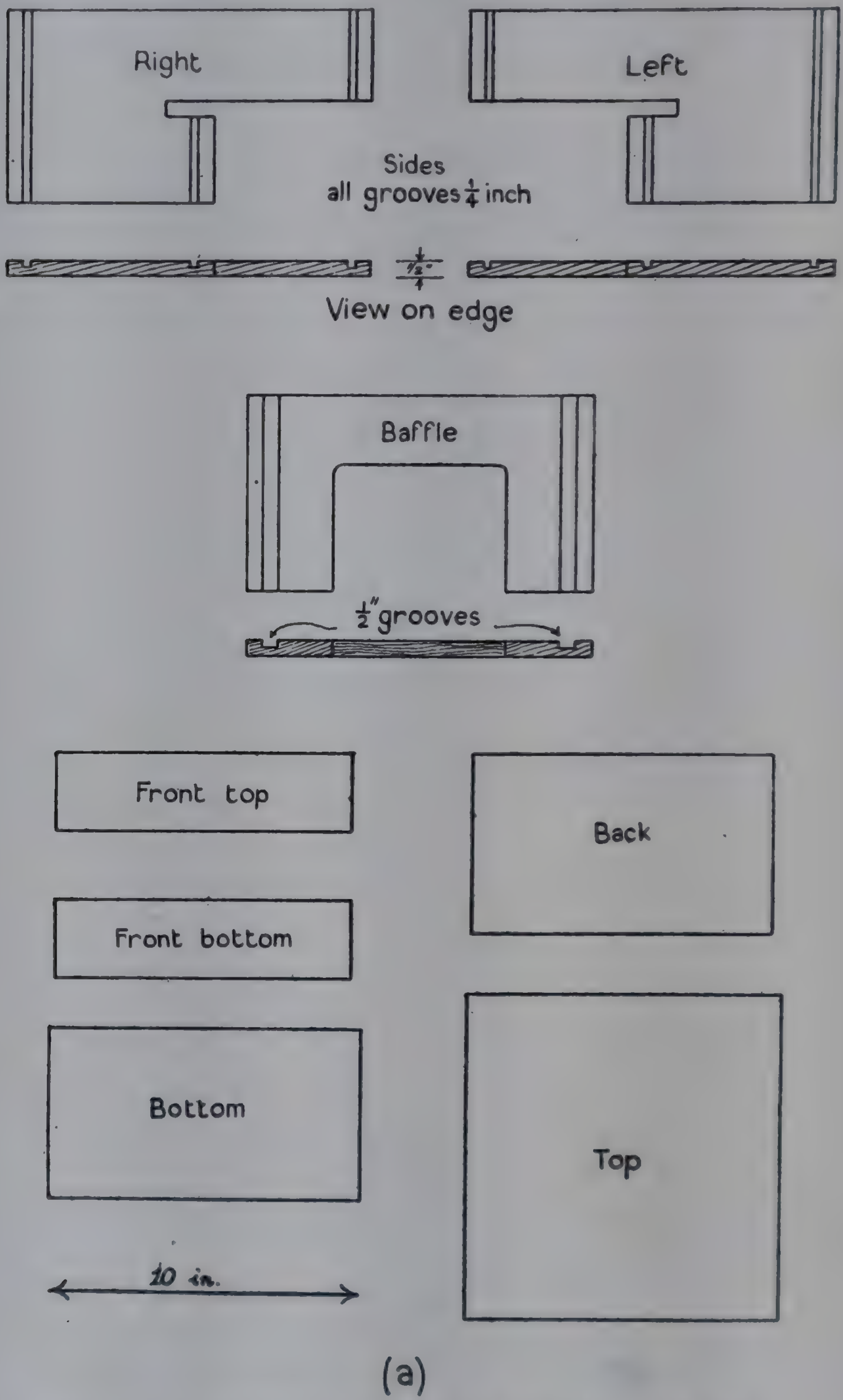

Right
Left
Sides
all grooves ¼ inch
½"
View on edge
Baffle
½" grooves
Front top
Back
Front bottom
Bottom
Top
10 in.
(a)

FIG. 1. (*a*) Component parts of the portable model of P3.

under it unless the shed is built on piles well clear of the ground. This seemed to suggest that rats would go under low tunnels (if the underside of the floor of a shed can be described as a tunnel), so some low wooden tunnels with no floors were made and tried. These proved very successful, the rats going straight into them.

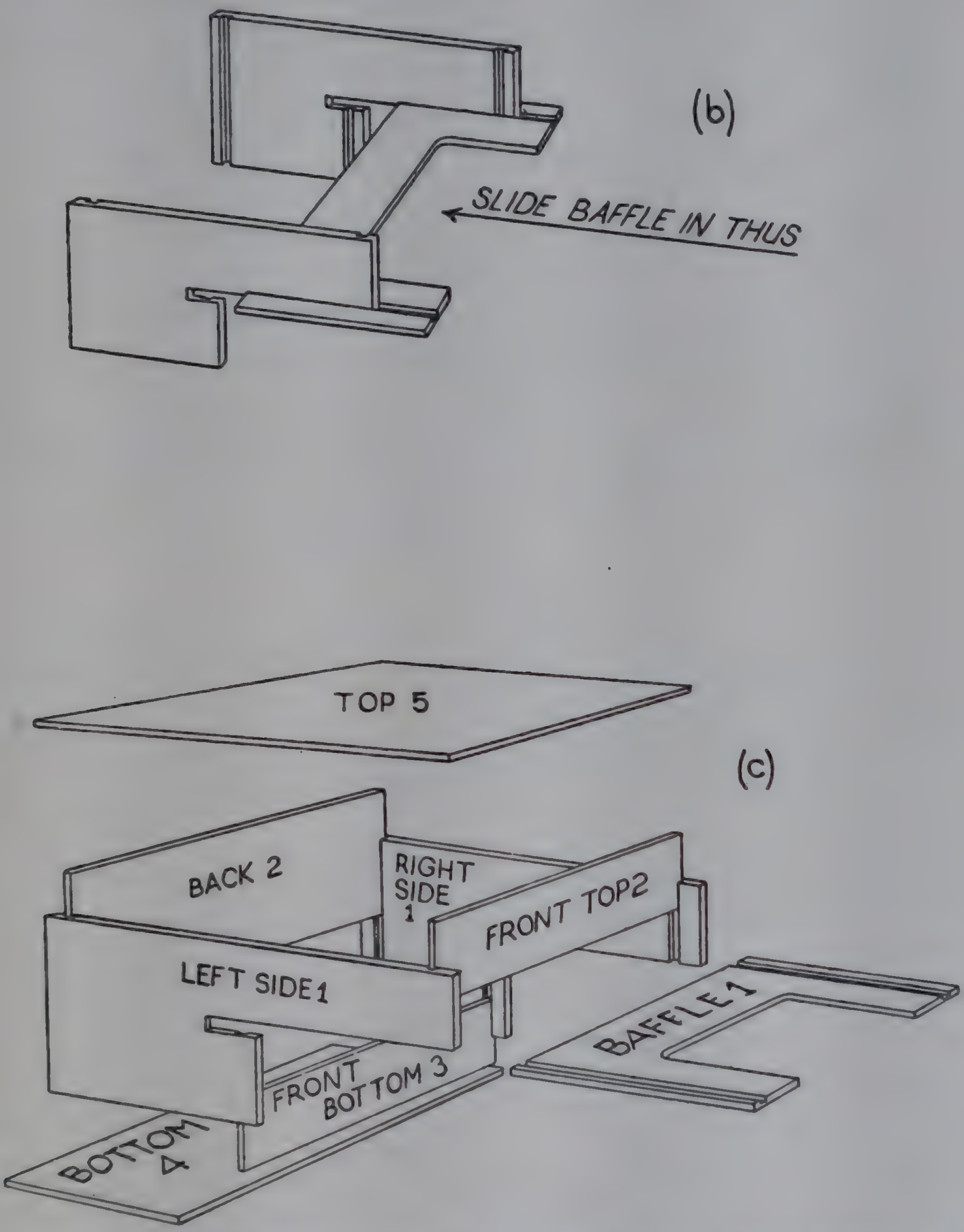

(b)
SLIDE BAFFLE IN THUS
TOP 5
(c)
BACK 2
RIGHT SIDE 1
FRONT TOP2
LEFT SIDE 1
BAFFLE 1
FRONT BOTTOM 3
BOTTOM 4

FIG. 1. (*b*) The first assembly operation. (*c*) The parts as they fit together (in the order shown by the numbers). (Reproduced by courtesy of the Agricultural Research Council.)

The 'go-up'. Turning again to the analogy of the shed, it is common for rats to gnaw their way through the floor from the underneath and to go up into the shed, wherever this is possible. It was, therefore, decided to erect a small 'shed' on top of the low wooden tunnel and make a hole in the roof of the tunnel for the rats to go up into. A ledge was fitted round the hole so that bait from the shed should not fall down through the hole and be left on the ground if the tunnel was moved. Despite this ledge some bait did get spilled by rats when they went up into the shed; to prevent this the ledge had to be made much deeper. The logical way of doing this, rather than increase the height of the shed, seemed to be to push half the shed over the edge of the tunnel and make it hang down to the ground in the form of an annexe. This was done and the P3 container in its present form was conceived.

The baffle. A tunnel is formed when a P3 is placed against a wall, and the roof of this tunnel extends back into the inner compartment for 1 in. This platform and the ledge that it forms is called the 'baffle'. The reason for making the baffle overlap the inner compartment is partly to make it more difficult for poison bait to spill out if the P3 is accidentally tipped over, and partly to add to the rat-specific qualities of the container by keeping out long-necked birds.

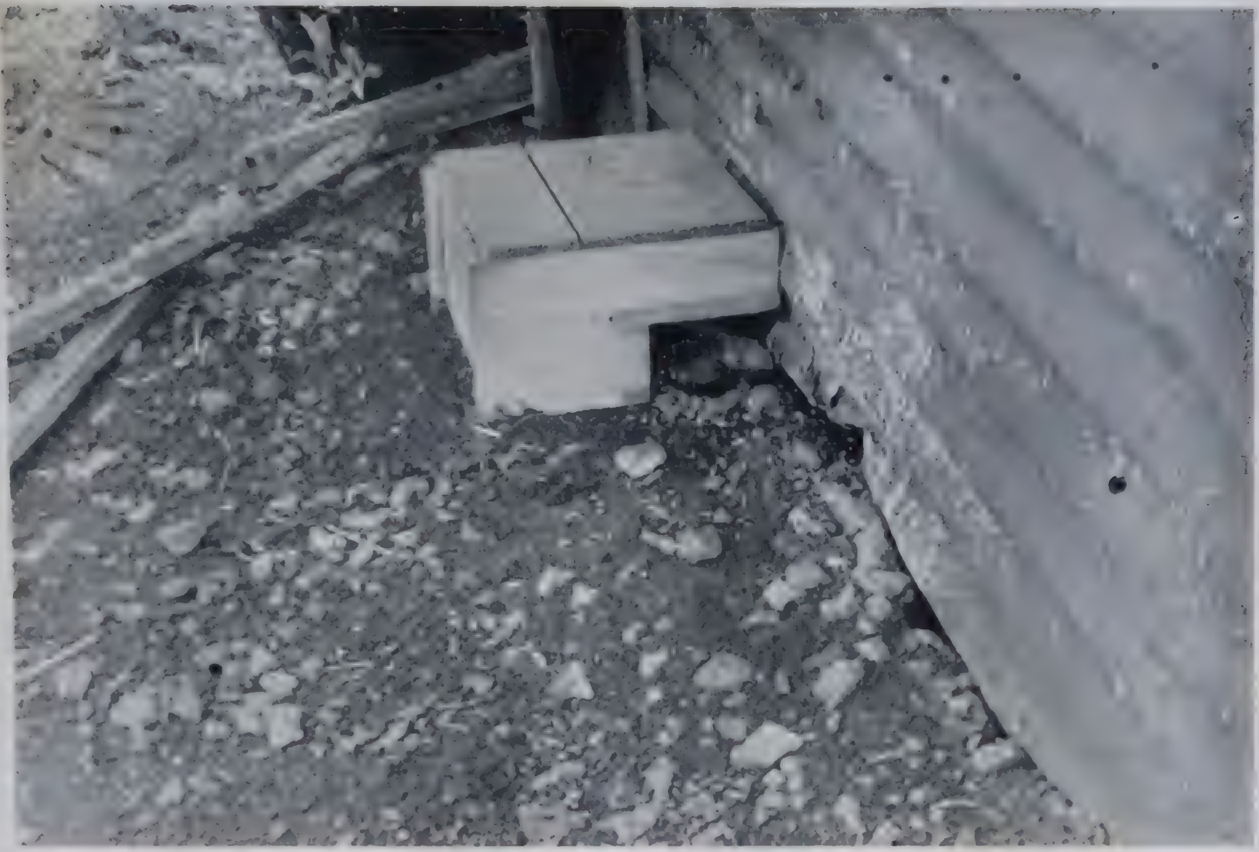
P3s are used both indoors and in the open. If used out of doors they should have a piece of roofing felt or similar material nailed on to the lid, with some overlap, to prevent rain soaking through to the inner compartment.

(*b*) **Safety**

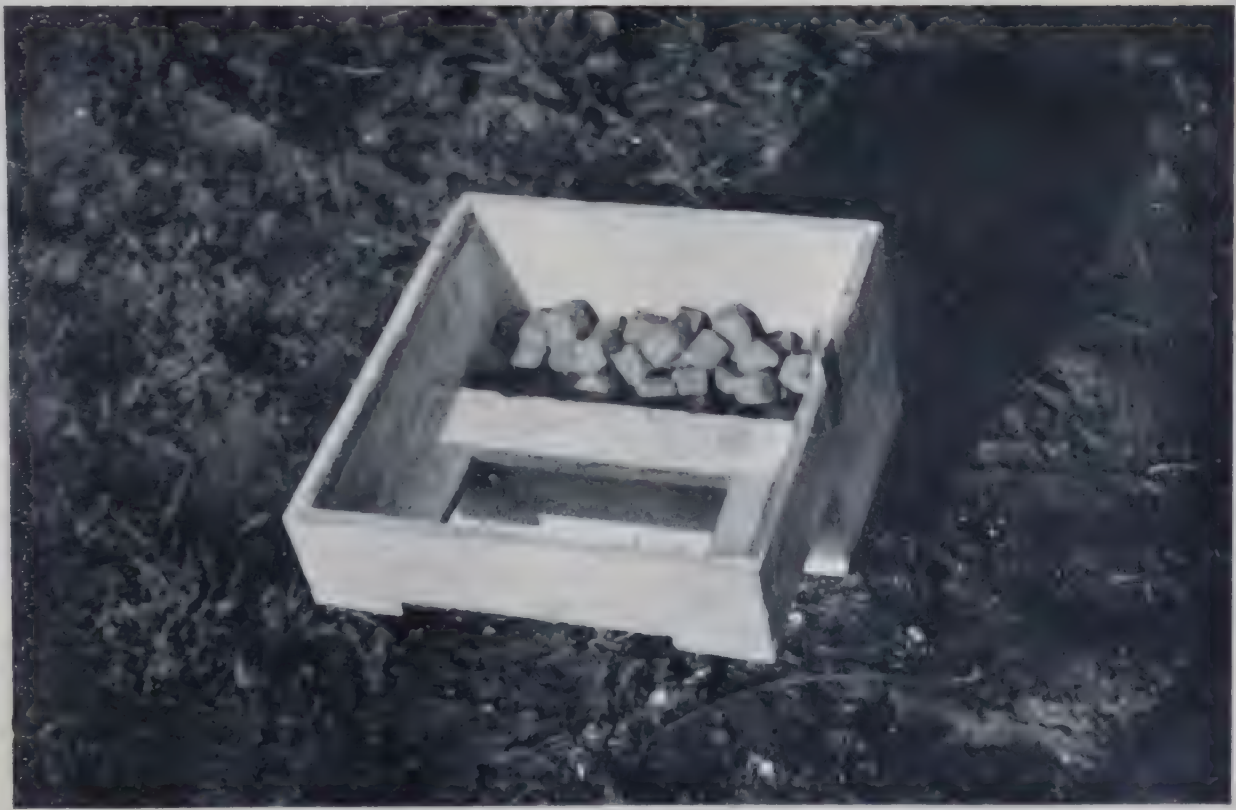
There is no doubt that (subject to some qualifications that are discussed later) rats and mice readily enter P3s when they have become accustomed to their presence in the environment, but that other mammals and birds do not or cannot do so. In the light of 6 years' experience, it can be claimed that the combined effects of the tunnel, the go-up, and the baffle, are to give a container which is almost entirely specific for rats and mice: the sizes, athletic limitations, and habits of other animals making it so. P3s have been observed on poultry farms and while chicks will occasionally go through the tunnel they have never entered the P3s nor taken any bait from them. Sparrows have, however, twice been observed to enter P3s. No complaint of the accidental poisoning of any domestic animals has occurred during the whole course of our work with P3; though it should be noted that no amount of precautions in covering in poison bait can prevent the risk of illness or death in domestic animals such as dogs and cats if they happen to eat poisoned rats dying or dead in the open. This risk is most likely to apply to the use of zinc phosphide, which causes more surface death of rats than does arsenic, and which has been mainly used in the later period of the Bureau's research when P3s were less widely employed.

To minimize interference by children, a simple locking spring is added that is too strong for a child to open. Interference by older people can be reduced by stencilling a warning on the lid of the box.

Safety to man depends not only upon the inaccessibility of the bait inside

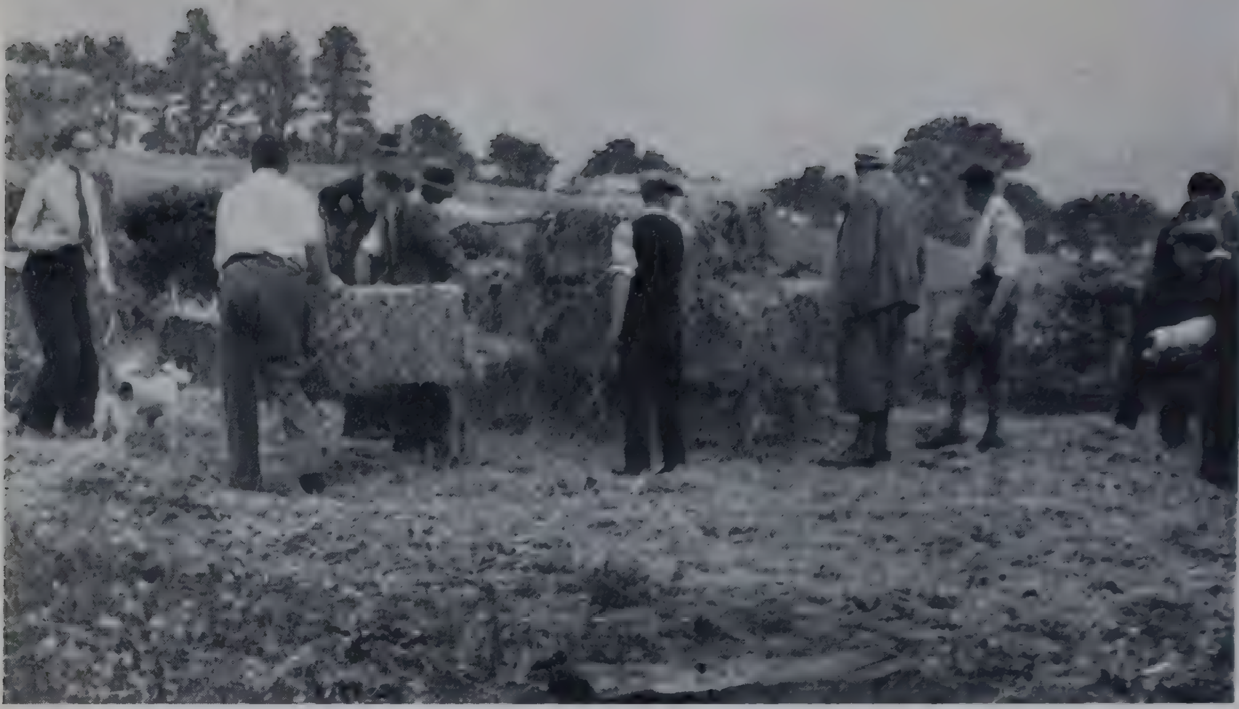


(*a*) P3 newly placed and baited

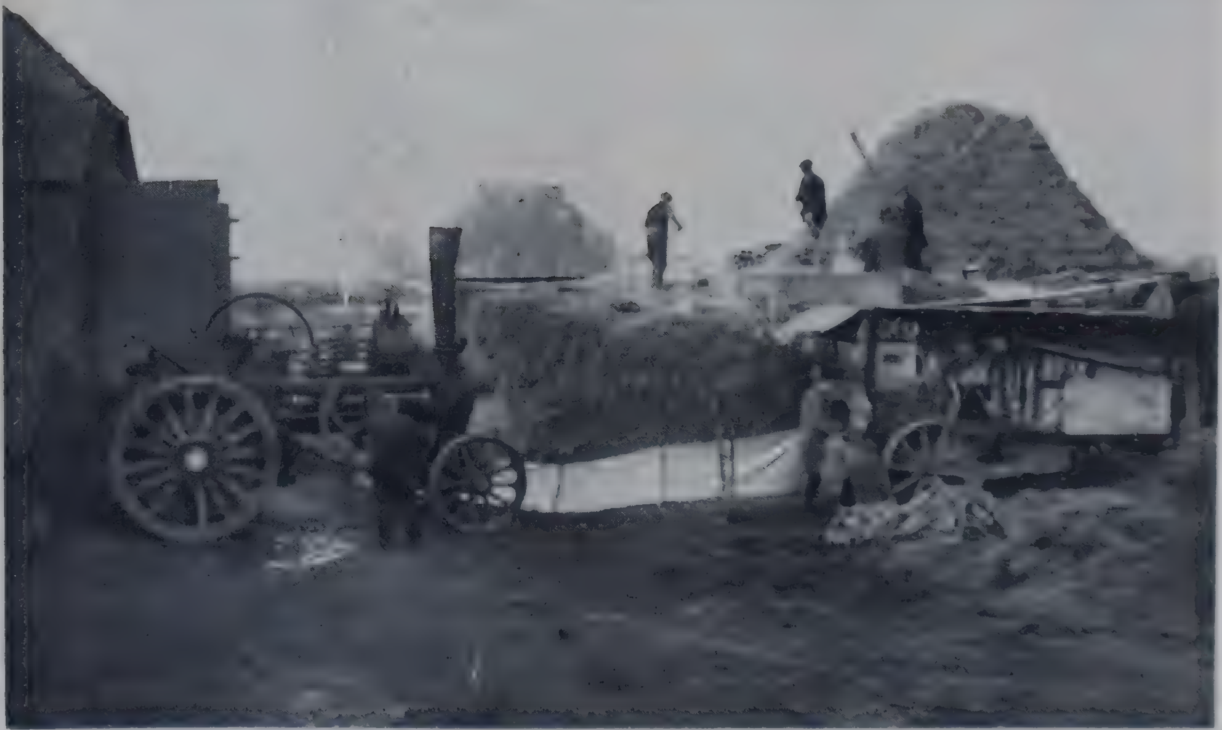


(*b*) P3 containing 1 kg. dairy cubes carried in by rats in a single night

PLATE 1



(*a*) Recovering poisoned rats from straw bales, Worcestershire



(*b*) Threshing a corn-rick, Oxfordshire

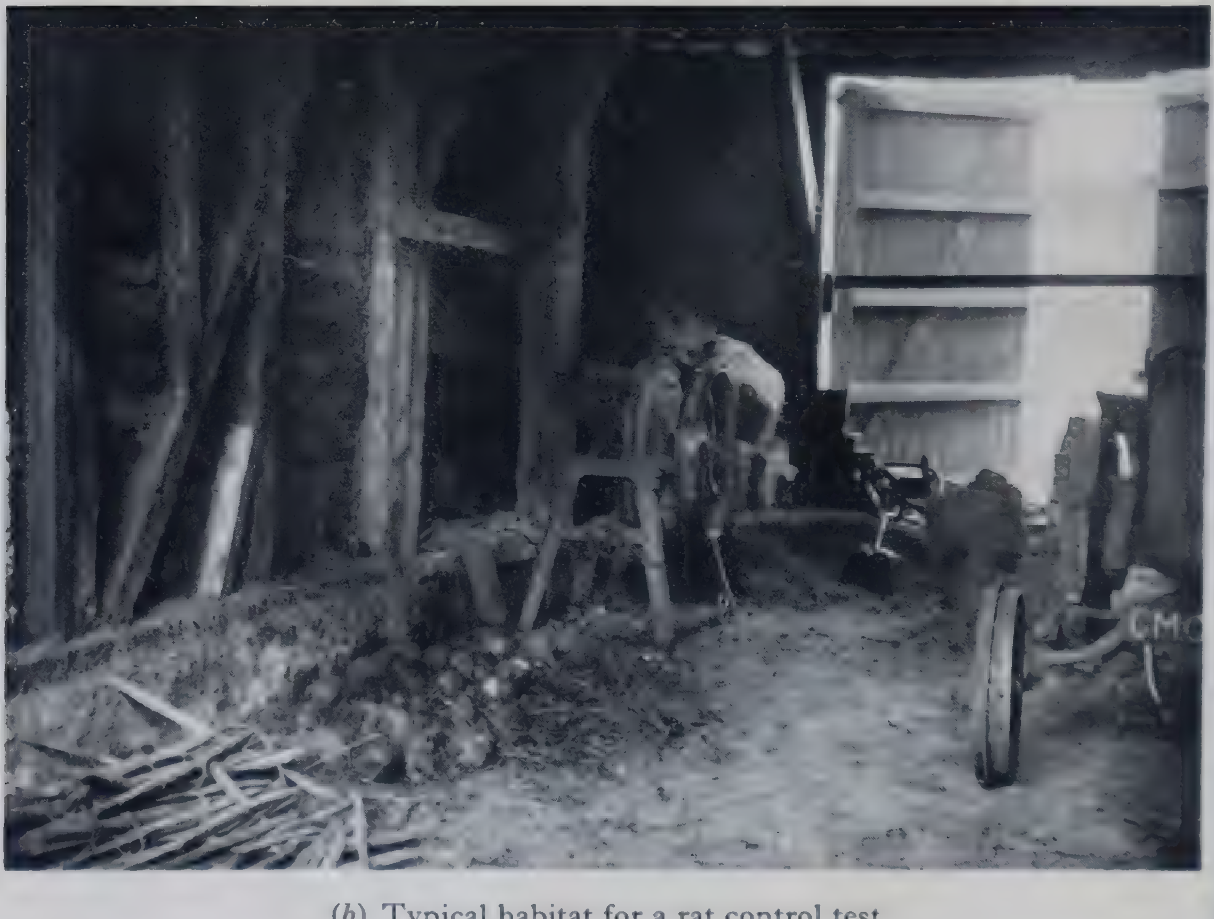
PLATE 2

(*a*) P3 census point. Tangles such as this make it difficult to estimate rat abundance except by a baiting method

(*b*) Prebaits placed (for illustration only) at the mouth of rat holes during a practical operation

PLATE 3

(*a*) Small pig farm: site of an experimental colony of wild rats



(*b*) Typical habitat for a rat control test

PLATE 4

the P3 but also on the extent to which it can be spread around either by accidental spillage or by the rats carrying it away. The spillage is well controlled by the structure of the inner box with its baffle ledge. Removal and scattering or storage elsewhere of the bait is greatly reduced by the use of comparatively non-portable types of cereal bait, such as grain, rusk, or sugar-meal, of which only one mouthful at a time can be carried away. Rats do frequently carry away mouthfuls of bait out of a P3 and eat them a short distance away, though this involves comparatively little dispersal of dangerous material on the floor or ground. (Continuous feeding also takes place often inside the P3.) Whatever the amount of risk arising from this cause, P3-baiting is a far safer method than surface baiting in places where baits might easily become mixed with human food or be accessible to stray animals or children.

British laws forbid the exposure of poisoned baits on the open surface of land or, indeed, in any place where reasonable precautions have not been taken to prevent access by dogs, cats, poultry, or other domestic animals (Protection of Animals Act, 1911, Section 8), or wild birds (Protection of Animals (Amendment) Act, 1927, Section 1). The use either of P3 or of carefully done hole baiting would appear to meet the intention of these laws, though one cannot pronounce on such matters without a study of relevant case law, which for P3 does not yet exist.

(*c*) Patenting and manufacture

A Provisional Patent Specification for P3 was filed at the Patent Office in June 1940 in the names of Elton and Ranson as 'inventors' (the latter being the actual inventor, and the former handling the business side), under the title of 'Improved container for rat poison bait'. The patent (No. 541,844) was completed on 15 December 1941 by Imperial Chemical Industries Ltd., to whom the rights had been assigned, and who agreed with the inventors to hand over the patent rights to the Government if requested. This was eventually done in September 1943, when the A.R.C. took over the patent rights which they still hold and which under normal circumstances will remain valid until at least 1957.

It should be mentioned here that the handling of the patent transactions and the adaptation of P3 for practical manufacture would have been almost impossible but for the generous and sustained help given by Dr. R. E. Slade, the Director of Research, and his assistant Mr. D. J. Branscombe, in Imperial Chemical Industries. The fertility of Dr. Slade's ideas and the commercial knowledge of him and his staff were of signal value during the development stage of this project.

Manufacture of P3s during the war (and still after it) has been severely handicapped by shortage of suitable wood. This was especially so because it had been decided that they should be made in parts that could be assembled by operators on the spot, and this demanded strong seasoned wood not likely to warp and with reasonably smooth texture. When assembled as a box, P3 is rather bulky. It is not easy to carry more than four or five at once, and two dozen almost fill up the back of a small car. To transport a gross requires four enormous packing cases, and takes up a very large space in the van. If the P3s are made sectional, the pieces can be laid together flat, and they take up very little space. Several designs for a sectional P3 were thought out in collaboration with Dr. Slade and Mr. Branscombe. The final design (Fig. 1), which is made of wood throughout, is built up on a girder system formed by locking the baffle and the two sides together, by sliding the sides into grooves cut in the baffle. This girder system, which is the foundation of the sectional P3, was first suggested by Dr. Slade and developed afterwards by the Bureau. The rest of the sides slide into grooves cut in the sides which form the girder, and the bottom is nailed on (4 holes and 6 nails being provided). Finally, the lid fits on the top of the assembled P3 and is held in place, if necessary, by two leaf spring catches. Theoretically, the only tool required to assemble the sectional P3 is a hammer. In practice, the poor wood supplied caused a good many jams and breakages during assembly. Several alternative models made of substitute materials such as plaster board were considered but were abandoned for various reasons.

The M.O.F. has so far been the only large buyer of P3s in this country. They were made on licence at first by the Rawlplug Company Ltd. and later by the Ideal Window Company Ltd. So far over 7,600 have been made and distributed by the Ministry (recent cost 4*s*. each), mainly to their own divisional rodent officers and to local authorities. Five hundred were sent to Malta in 1945 to assist the rat campaign during a small plague outbreak on the island. The Ministry kindly made over several hundred for the research of the Bureau of Animal Population. A licence for manufacture has also been granted to the Department of Public Health in the Union of South Africa and to African Explosives and Industries Ltd. in the same country.

(*d*) **Behaviour of rats**

(i) *Lag in entry*

It is shown elsewhere in some detail (Ch. 5) that brown rats are usually very sensitive to changes in the spatial arrangement of their environment, and have a marked 'new object reaction'. This is well seen in their frequent refusal to enter containers when these are first put down. There is usually a lag of from 1 to 10 days before entry occurs. It is sometimes longer. The normal procedure is to place the containers in the exact positions required for poison baiting and leave them for at least a week before regular pre-baiting begins. Entry is speeded up if the tunnels of the boxes are baited when the containers are put down. It is very important not to move the

P3s even a few inches from their original position, while the operation is in progress.

Lag is a serious drawback to the use of containers for practical rat control work, because it usually means an extra journey by the operators in order to place the container at first, and this increases expense of labour and transport. That is partly why the system of baiting inside existing holes and crevices occupied or visited by rats is now the general method on farm areas. For it is in rural rat control that time and transport are especially limiting factors, but here also rat holes are relatively numerous and accessible. Thus the 'natural P3s' of the rats can be baited, and the only lag to be overcome is new object reaction towards the bait itself, and this can be achieved in 2 or 3 days' baiting.

In towns, rat infestations tend to be more closely grouped, and at the same time human occupation is more intense and the primary holes of rats much more often inaccessible. Here containers are of the most value; though it is also often possible to carry out surface baiting where the rooms can be closed off; and, of course in sewers, where there is no danger of interference by species other than rats. P3s have not proved successful at corn-ricks or in bulk cereal stores.

(ii) *Use of cover*

Once lag has been overcome, some rats at any rate seem to find P3 attractive, since they will visit it even in the absence of any bait. Numerous watching records by members of the Bureau prove that although rats feed in the open, on the whole they keep under cover in the day-time. Their behaviour in the dark is less well known. An experiment by Ranson with three P3s, one at a slaughterhouse and the others on a pig farm, illustrates the attraction of the boxes to brown rats. After entry and the bait being eaten by the rat, no more bait was put in. Rat visits to the slaughterhouse P3 (determined by footprints in powder) continued for 11 days and then ceased. At the pig farm both boxes were regularly though not continuously visited for 95 and 117 days respectively, no bait being put in during the whole of this time. These and other instances of the kind, suggest that rats will to some extent go into P3s for their own sake, presumably because they provide safe cover.

(iii) *Storing*

During their visits to P3s, rats sometimes store up large quantities of material. For instance the following articles, among others, have been brought into P3s from time to time:

Pieces of gristle.
Remains of dung beetle (*Geotrupes stercorarius*).
Orange peel.
Slices of bread and butter.

A government form dealing with swine.
Empty shell of water snail (*Limnaea stagnalis*).
Many partly chewed earth-worms (on one occasion 27 g.).
Pieces of fish skin.
Potatoes.
Stems and leaves of elder (*Sambucus nigra*).
Dead young rat (*R. norvegicus*) slightly gnawed.
Willow (*Salix*) leaves.
Knuckle bone.
Leaves of plantain (*Plantago media*).
Lumps of suet.
Empty packet of 'Woodbine' cigarettes.
Piece of electric cable.
Piece of ox stomach.
Sheep's wool.
Head of a starling (*Sturnus vulgaris*).
1,000 g. 'Dairy nuts' cattle cake brought in during one night (Plate 1).
27 g. cotton seed cake brought in during one night.
General rubbish such as grass, stones, hay, straw, sticks, &c.

This strange collection of objects from civilization and from nature does suggest that some rats regard P3 as a safe storage place and a refuge.

(iv) *Relation to baiting efficiency*

The whole question of how rats behave towards containers, and the efficiency of container baiting, is treated in Chapter 4. Here it will only be noted that container baiting, even with non-permanent baits and the best methods of setting and prebaiting, does not give such consistently good results as surface baiting and hole baiting, at any rate with large populations. This is thought to be due to a variety of factors, not all of which are understood. But it is known that (1) in certain 'difficult' environments such as grain stores, P3s may not be entered at all, (2) sometimes P3s do not draw in the whole of a rat population, even though they are in contact with it, (3) there may be 'queueing' for entry that would slow down feeding by some rats, (4) being fewer in numbers than hole baits, containers may not be placed at enough good points to make contact with the whole population.

On the other hand, the specificity of containers, especially P3s, is a very valuable property, and they can also protect baits (including prebaits) from the effects of weather. For experimental work they enable essential measurements of quantities of bait to be recorded, a proceeding impossible in hole baiting and less convenient and accurate with most surface baiting. For further discussion of these practical points reference should be made to Chapter 4.

3. PIPES

While testing rat poisons on refuse dumps in 1941 it occurred to Elton that experimental and practical control might be easier if an 'artificial rat hole' could be offered, in which bait could be placed. This led to the use of drain pipes buried in the banks, and it was found that rats entered readily once they had become accustomed to them. The most handy and universal pipe is the small unglazed agricultural 'mole drain' pipe, length 15 in., inside diameter 3 in., with a fluted outer surface and with one end specially blocked with a plug of cement. One of these pipes will safely hold about 500 g. dry wheat, i.e. enough to prebait fully about twenty 'average' rats. It was soon found that rats would enter these pipes just as readily if they were laid on the open surface (the fluting helping to prevent rolling), and they have been used in this way for census baiting, and some poison baiting, in a large number of experiments on farms, refuse dumps, and elsewhere. They can be laid singly or in groups ('batteries').

Pipes have two disadvantages compared with P3s: they are heavy to carry and are not so specific to rodents. The bait sometimes spills out, but usually not much if the poison bait is placed well at the back and the pipe very slightly tilted up in front; and poultry sometimes poke their heads in and eat the bait. Small passerine birds (sparrows, starlings, &c.) have only very seldom been seen to enter the pipes—indeed flocks of sparrows have been observed to ignore completely for weeks pipes almost full of wheat, some of which was scattered outside. The danger to poultry can be reduced by facing the mouth of the pipe 3 in. from a brick or stone or upright surface; another commonly used method is to tilt a board over the ends of the pipes and weight it with a brick.

Rats show a lag in entering pipes similar to that with P3s, and much of the reasoning given above about the use of P3s applies in general to pipes.

One of the chief advantages of these pipes is that they are an article in universal use throughout the country for land drainage, and they can be converted into bait containers very quickly by the simple process of plugging one end with an inch of cement. They are also much cheaper than P3s, costing only about a shilling each, including conversion. Pipes are also less liable to attract the attention of people likely to interfere with or steal them. Buried pipe batteries survived many weeks on a refuse dump visited by scores of people and children in search of loot, though some interference has occurred in other instances.

4. OTHER BAIT CONTAINERS

The idea of specificity in poison bait containers is not in itself new, either in the control of rats, or of field mice. Several different main ideas have been adopted or recommended, the following references being primarily to rat control.

(*a*) Hollow lengths of bamboo have been used in several tropical countries, as Java (Dammerman, 1929, p. 289), British Malaya (South, 1931; Beeley, 1939), Mauritius (Mauritius Dept. of Agriculture, 1922), and Jamaica (Anon., 1920). The protection provided by such 'pipes' is naturally only partial, and a major motive in using them is to shelter baits from the weather, especially heavy tropical rains. The metal half-cylinders used by Doty (1938) during prebaiting and poisoning in Hawaiian sugar-cane fields are purely to protect from rain; and Beeley's (1939) and Cleare's (1938) more elaborate metal shield is mainly for the same purpose.

(*b*) Boxes with holes of the right size to admit a rat have been recommended by several people in recent years in the United States (Hamilton, 1936; Silver and Garlough, 1941; U.S. War Department, 1945), also by the makers of Zelio, a thallium sulphate paste (Bayer Products Ltd., n.d.) and of Rodine Phosphorus (Thomas Harley Ltd., n.d.). Silver and Garlough print a construction diagram of the box recommended by the U.S. Fish and Wildlife Service in 1941. Coogan (1945) used a narrow wooden tunnel, 20 in. long and 2×4 in. inside dimensions, as a protection for dogs and cats, in a rat-clearance project in Massachusetts: this is recommended officially by the U.S. War Department (1945, pp. 67–70). This is clearly the type of box mentioned by Truman (1945). Some simple ideas of this kind were used by the M.O.F. (1942) in publicity for their first rat campaign in the Second World War. None of these boxes gives safety for birds as well, though some of them are cheaper and easier to make than P3. The German metal box with holes in the sides for baiting voles, figured by Storer (1938, p. 50), embodies the same simple principle as the rat boxes.

(*c*) Kelly (1894), an American chiropodist and inventor, patented a variety of metal containers which he described as 'to be used with poisoned food or water, for the destruction of vermin as rats, mice and sparrows'. It will be seen from the wording of his patent that he did not intend his containers to be specific for small rodents. But in some of their features they come near to P3, which is not absolutely specific against sparrows. A metal box having some of the features of P3, but more closely resembling Kelly's designs, was used for rat destruction on the London and North Eastern Railway for some years.

5. SUMMARY

1. A wooden bait container specific for rats and mice was designed and patented and tested in the field.

2. The original idea was to use the containers for a system of permanent poison baits, but this proved impracticable because of the behaviour patterns of wild rats in relation to poison baits. The containers, however, proved valuable for research tests of baits, and to a certain extent for prebaiting and poisoning rats in certain habitats. They are called P3—'Protected Poison Point'.

3. There is usually a delay before rats enter P3s placed in their foraging area. After this many rats enter freely, and sometimes use the boxes for storing various objects. (*R. rattus* probably has less lag than *R. norvegicus*.)

4. Another bait container, made from a small drain-pipe blocked at one end, has also proved efficient in the field, but with much the same limitations as the P3.

5. Some other types of bait containers for rodents previously tried by various people are noted.

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4

THE STUDY OF THE BROWN RAT AND ITS CONTROL BY POISON

By DENNIS CHITTY

I. GENERAL INTRODUCTION

(*a*) Aims

RAT control is a technology and its study therefore differs from a scientific investigation in having limited and practical aims. The success of any such study is determined by the extent to which these aims are met and not by any contributions which may be made towards science. It is rare, however, for any complex technological problem to be solved except through recourse to scientific methods; but the more economically the problem can be solved the fewer will be the facts of general interest. While the present chapter is therefore concerned with a number of studies of limited value, others differ only by their incompleteness from those that would have been undertaken if our problem had been the rat itself, rather than its control.

The first concern in either case must be to develop techniques for studying natural populations. With a practical problem we must then use these techniques in order to develop a further set of techniques to control the pest, whereas in a scientific investigation we will be concerned with purely biological problems. However, unless we are lucky enough to apply a successful control method at the first attempt, the practical investigation will proceed along lines which differ from the scientific in degree only. It did in fact prove necessary to find out a great deal about the biology of the rat, not only to improve the methods of control but to improve the methods of assessing them. For both purposes we chiefly needed to understand the factors affecting the feeding behaviour of the rat, and the double use which could be made of this information was indeed fortunate, except in so far as the results have proved more difficult to present. An arbitrary division of the data has been made into those which had most influence on the study of populations and those more relevant to problems of control.

(*b*) Acknowledgements

All my colleagues have contributed towards the information discussed in this chapter. I myself have done few of the tests in Appendix 1, the chief contributors being A. D. Middleton, Monica Shorten, H. V. Thompson, and L. S. V. Venables. Special investigations are acknowledged individually, but I also wish to thank Monica Shorten and H. V. Thompson for help with the rats in the outdoor cages and at the experimental colony. The late R. M. Ranson helped with and inspired so much of my own work that his death was a particularly severe blow. I owe much to Charles Elton, not least for the editing of the interim reports, which have been invaluable in preparing the present account; to R. B. Freeman for help with the literature and to many who have read the manuscript. Professor C. G. Douglas gave me generous advice with the feeding experiments and has kindly read the account of them (§ 7).

PART 1. METHODS OF MEASURING RAT POPULATIONS

2. INTRODUCTION

In order to test the efficiency of a control measure it is necessary to compare the population density of the animals before and after treatment. Rat control has often suffered from the lack of an objective approach and before much progress could be made in our own work it was necessary to correct this situation. Methods evolved with increasing knowledge about rat behaviour and some of our earlier evidence proved inadequate. This is not an unusual event, for it may take several years to develop a new field technique, and its faults may not be apparent until it has been used to find out a good deal about the animal. Part 1 of the present chapter is chiefly concerned with problems of 'census'. Since this is a word which occurs rather frequently some explanation of its use must be offered.

The historical meaning of 'census' is a direct count of each person in an area at a certain moment of time and the term is sometimes used in this precise and limited sense by human demographers (Deming and Stephan, 1941). Similarly, in other fields of study, e.g. in agriculture, 'the census differs from the various estimates of crop production, live-stock, etc., in that it is based upon a direct count of all the items represented, rather than upon a sample or upon calculated probabilities' (Truesdell, 1929). In this sense, therefore, a figure based upon enumeration of anything less than all the individuals is not a census but a sample estimate of the population.

Attempts at the complete enumeration of wild animals may encounter great practical difficulties, and an apparently simple instruction such as 'birds flushed more than once are not recorded' is not necessarily easy to apply. Indeed, the recording errors of a complete enumeration may be comparable in magnitude with the statistical errors of a smaller sample and there may then be no advantage for the census (Finney, 1947).

The exact use of the word 'census' is not often adopted by biologists, and many understand the phrase 'census methods' to include a variety of ways of obtaining data about animal populations. The farthest departures from the restricted meaning are shown in phrases such as 'relative census' used by myself (1942), and 'questioning the people' which is included by Emlen, Stokes, and Winsor (1948) among their 'census methods' for rats. The various meanings that have been attached to the word 'census' are as follows:

1. Complete enumeration (100% sample)
2. Partial enumeration (<100% sample) } = population estimates.
3. Index of absolute numbers }
4. Index of relative numbers

The original definition would restrict the word to the first category, and some confusion might be avoided. Most biologists, however, would include the second definition and many would include all four. The word 'census' has thus been used to mean any quantitative measure of absolute or relative numbers. To the statistician, a return to the precise use is desirable for reasons which Dr. Finney has stated to me as follows:

> The logical difference between the aims of a complete enumeration and of any other method of quantitative population study are sufficiently great to deserve a difference of name. The present practice obscures the distinction between errors arising from mistakes or inconsistencies in enumeration and the statistical errors inherent in estimation by sampling. Furthermore, the type of index [No. 4 above] that cannot be used as a basis for an estimation of absolute population size, should be excluded not only from any meaning of 'census' but also from 'sampling', unless accompanied by explicit statement of the biases from which it may suffer.

I have found it easier to see the justice of these arguments than to find a convenient alternative to the use of 'census' in its widest meaning. While it is clear that 'index' could have been substituted in many cases, in others there is no alternative but to use a phrase which generally complicates rather than clarifies a terminology that has proved useful throughout our work. Consequently it has seemed advisable, for the time being, to state the issue without attempting to resolve it.

3. REVIEW OF VARIOUS METHODS OF STUDY

(*a*) 'Census' by killing

In all cases where a sample of animals is killed and counted, the numbers taken must be small in comparison with the remaining population if subsequent samples are to be drawn. There is seldom any likelihood of affecting populations of plankton or wireworms in this way; but at least with mammals and birds this is a danger unless large areas are available for study. Dopmeyer (1936) and Doty (1944) used the results of trapping over large areas to show the effect of rat control; but in our own work it was seldom possible to trap without seriously reducing the population it was wished to study. There are also such unknown factors as trap-shyness which may affect the validity of later catches. Where traps can be concealed in the runways, suitable trap census methods might be developed; or numbers bolted with gas, &c., could be compared per unit effort. Our own attempts along these lines showed that these methods were, at best, of strictly limited application.

Corn-ricks (Ch. 11) are among the few habitats suitable for a census which involves killing the rats, and since the habitat is destroyed in the process only one census is possible. This is carried out by counting the number of live and dead rats at threshing. As a rule the rats are part of a colony which also occupies the ground nearby and which must be poisoned at the same time. Corpses found in the open near the ground-living part of the colony must therefore be excluded from the total attributed to the poisoning of the rick. Only an arbitrary correction can be made for this error, and it must be assumed that the numbers alive and dead in the rick, together with those dead in its immediate vicinity, are a fair sample of the effect of the poison. There are various other errors: some rats escape unobserved into the litter round the rick, and when operations are interrupted by rain or nightfall there is no way of preventing rats from leaving the half-finished rick.

(*b*) 'Census' without killing

Where populations are of limited size, or it is otherwise undesirable to kill a sample, a relative count may be made of individuals seen per unit of time or distance. In other cases it may be more practicable to obtain an index of abundance from the droppings, tracks, burrows, damage to vegetation or structures, or the proportion of ground occupied by these signs. Methods of this general type (Dice, 1941; Chitty, 1942) which have been adapted to rat control research are as follows.

(i) *Counts of individuals*

In some environments rats can be counted by day during their normal activity or when attracted to baits. However, the day-time activity of a reduced population may be disproportionately less than its reduction in numbers because there is less competition for food, so that counting must nearly always be done at dusk or dawn or by flashlight at night. One count is made before the poisoning and another sufficiently long afterwards for its effects on activity to have passed away. The observer makes a transect of the infestation or stands for a certain length of time in one spot. In the latter case the same rat may be counted several times, since rats commonly move back and forth between food and cover. An extra and often considerable variable may thus be introduced. On the other hand, the transect method usually drives the rats to cover and makes it hard to estimate numbers when many rats are disturbed all at once.

There will also be errors due to variations in activity from day to day and according to the time of day. Comparative counts made on various evenings in markets in Baltimore show considerable variations (Emlen, Stokes, and Davis, 1949), and as shown in Table 31, activity at the experimental colony varied between 194 and 4 feeding visits in two successive hours. Nevertheless in suitable environments the method may be satisfactory (Table 1 and Coogan, 1945). The method, of course, gives no more than a comparison between original and residual populations and does not give information about relative numbers in different colonies. This is exemplified in Table 1 by the two tests in ricks where the dusk counts were about the same for both though one population was 5 times the size of the other.

TABLE 1. *Comparison between dusk counts and other estimates of kill*

Dusk count		*Reduction*		*Reduction*
Before	*After*	%	*Results by other methods*	%
17	4	76	Census baiting: (1) 3,025, (2) 490 g.	84
14	0	100	Some rats left	< 100
20	1	95	10 dead, 1 alive at threshing	90
19	0	100	51 ,, 2 ,,	96

(ii) *Tracks*

In many environments rat tracks may be distinguished in dirt, dust, or snow, but as individuals travel over the run many times each night it is difficult to distinguish between different frequencies of traffic. A treadle type of activity recorder was often avoided by the rats. Trace patches of flour, lime, plaster of Paris, &c., are good indicators of the presence or absence of rats and it is desirable (though seldom practicable out of doors) to check all supposedly complete clearances by this method.

(iii) *Holes*

The number of holes kept in use by a population of rats varies a great deal between habitats; but within certain colonies there may perhaps be a rough relationship between population size and number of holes maintained. Except as an accessory to other methods we have seldom relied upon hole-counting before and after poisoning. Before poisoning is carried out all holes are blocked with earth or paper and next day a count is made of the number reopened. Blocking and counting is repeated a few days after poisoning.

In buildings the entire population may be limited to the use of a few holes and survivors will also be obliged to use most or all of them. In such cases certainly, and probably in general, it is to be expected that the per cent. reduction in number of 'active' holes will be less than that in the population. This is true in the somewhat analogous case of sewer rat control where the proportion of manholes visited by rats is not reduced to the same extent as the rats are (Ch. 12). It would be unwise, however, to assume that the percentage reduction in active holes is always an under-estimate of the kill of rats.

In many colonies the number of holes eventually made is much greater than the number reopened on any one night. It is thus advisable to leave an interval between blocking the holes and carrying out the test. If this gap is omitted conditions are not strictly comparable with normal operations because of the difference in available baiting points and the chance that rats may burrow out at places where there is no bait.

A procedure which has great possibilities was carried out by Thompson. A hedge-bank infestation was divided into two parts and the number of baited holes was recorded without any previous blocking. One length of hedge was given standard treatment with zinc phosphide after prebaiting and the other was directly poisoned with Antu. In this trial the habitat was fairly uniform throughout, and as it extended for several hundred yards, the two parts of the population were fairly distinct. Two days after simultaneous poisonings, the holes were blocked; results on the next day were as shown in Table 2. These results do not show the percentage kill or reduction in active holes, but merely that the two trials gave approximately the same kill. In testing a new variable such a method might prove an economical preliminary stage, but in practice it is difficult to find infestations of sufficient size.

TABLE 2. *A paired trial*

Treatment	*Holes baited*	*No. reopened*	*% not reopened of those baited*
Standard . . .	488	30	94
Experimental . .	401	22	91

(iv) *Live trapping, marking, and releasing*

Total population may be found from the proportion of marked animals which are recaught or otherwise identified. The process is usually laborious and with rats may involve problems of trap-shyness and selection. After poisoning, the number of residual rats must be determined by a full repetition of the methods or by trapping out every survivor. McDougall (1946) has adopted this method. Our own experience has been limited to observing the proportion of marked animals visiting a bait point (Appendix 2).

Population size might also be estimated by marking and releasing a number of rats and finding what proportion they formed of the numbers picked up. It would be essential to deal with large populations and to use a poison such as zinc phosphide likely to produce many corpses. The method, illustrated in principle on p. 253, is worth trying on an adequate scale.

(v) *Other evidence*

General. An experienced field worker uses all available signs to guess the size of an infestation; but purely subjective evidence is of limited use. A kill of two-thirds or one half of a big population will obviously be attended by impressive results such as large takes of poison, plenty of corpses, reduction in damage and numbers seen. These facts are apt to mislead the operator about the number of survivors.

Attempts to count the number of clearly used holes or well-defined runs are subjective and suffer from the difficulty of distinguishing between heavy and light traffic. This type of approach has, however, been highly developed for work in city blocks in Baltimore (see below), and resembles in principle the method used for determining, mainly from droppings, whether or not a ship is badly infested. In the latter case estimates of numbers are based on a knowledge of the size of comparable populations previously killed at fumigation (Peirce, 1930; see also Mills, 1945). Methods of this type are not suitable for comparing results in dissimilar habitats.

Corpses. The number of corpses picked up after a treatment is commonly quoted as proof of efficiency. While this proves a certain minimum mortality and has a big psychological value (Eskey, 1934), no information is given about the residual population and little about the original one. Therefore, in the absence of other data, little importance can be attached to the size of the 'pick-up', which varies at least according to population size, kind of poison, and nature of the habitat. Even with standard treatments in the relatively uniform rick environment the proportion of the pick-up to the total kill is extremely variable. Table 3 illustrates how the pick-up is unrelated either to per cent. success or numbers killed: 5 times more corpses were found in the less successful half of this series of tests (see also Fig. 9).

TABLE 3. *Lack of correlation between success and number of corpses found (From Table 4, direct poisoning with zinc phosphide in rusk)*

R.C.T.:	*812*	*809*	*816*	*811*	*808*	*Av.*	*810*	*817*	*813*	*814*	*815*	*Av.*
% success . .	100	92	83	77	75	*85*	65	63	45	41	24	*48*
No. rats killed* .	16	9	34	13	16	*18*	24	25	12	20	12	*19*
No. rats picked up	0	0	1	3	1	*1*	2	4	11	7	2	*5*

* Estimated from reduction in take of census bait and on the assumption that each rat was taking 25 g. (Ch. 6).

Macchiavello (1946) compares the numbers of corpses picked up after different baits of '1080' to show their 'relative activity'.

Take of poison bait. The disappearance of poison baits is often considered to be a sign of efficiency. There are two main fallacies in this argument: (1) certain kinds of poison baits, especially those made up in bread cubes or biscuits, are carried away by rats; others of the meal type may be scattered or trampled into the dirt (§ 6 (*a*)), (2) even an accurate measure of poison take is useless as a criterion of success unless it can be used to show the size of the population and unless the proportion of survivors is also found.

Long (1931) calculates that four million rats were destroyed in one campaign where, out of twenty-one million baits distributed (70 tons), one-fifth to one-sixth of the individual packages was eaten. Cilento (1936) quotes 'positive and successful results' on the basis, for example, that 930 baits were taken out of 1,000 laid. Coogan (1945) calculates numbers killed from total take and average stomach contents of a sample of rats.

Satisfaction is frequently expressed when poison baits have been entirely consumed, although in this case there may not have been enough to go round. A different type of fallacy is to believe that if less poison is taken during a follow-up treatment with the same bait the population is correspondingly less than it was the first time. This argument neglects the effects of shyness among the survivors.

(*c*) **Other techniques**

(i) *Miscellaneous*

Dyed baits. Information about movements can be obtained through the use of dyed baits. Chlorazol blue is highly palatable (Ch. 2) and as a rule the dung is easily recognizable when 0·5% dye has been ingested in a wet bait such as bread mash. Eosin may be used at 0·5%; gypsum at 14% gives an almost pure white dung. Venables, especially, has used baits of all three types in different parts of one habitat. Both blue and red particles can be seen in the faeces of rats which have fed from separate baiting points; but gypsum becomes indistinguishably dyed if a coloured bait has also been visited.

During prebaiting with bread mash at a single site on the face of a rubbish dump pellets stained with chlorazol blue were found 100 yds. west, 60 yds. east, and 88 yds. north. Fresh, uncoloured dung 10 yds. from the bait showed that one rat, at least, would not be prebaited (see also Ch. 12).

Davis, Emlen, and Stokes (1948) have reviewed the literature on movements and themselves used methylene blue to trace the feeding range of prebaited rats. They found no blue faeces more than 100 ft. from a bait. Calhoun (1948) found comparatively restricted movements among similar colonies.

Corpse distribution. Some information about movements is also given by the distance at which corpses are found from the nearest poison. It is not necessarily true, however, that rats found dead near the bait were killed before they had been able to move away. Baits are generally laid where rats are most likely to be at any time of activity, and in any case, they may have returned after going elsewhere. For example, a rat was once found dead in a P3 in which the arsenic bait had been replaced by wheat 2 days before.

Distribution of takes. Movements can also be studied by comparing the amount of bait taken from different points in the habitat. The route of reinvasion of a sewer system was found over a period of weeks by noting progressive visits to new points (Ch. 12). Bait points can also be removed (or others added) and differences noted in take elsewhere. In this way a bad infestation in a grocery was shown to be derived from adjacent premises, for when bait was removed from the latter, takes went up in the grocery (§ 16). Some caution must be used with this method, for movements may be the direct result of new feeding habits among baited rats.

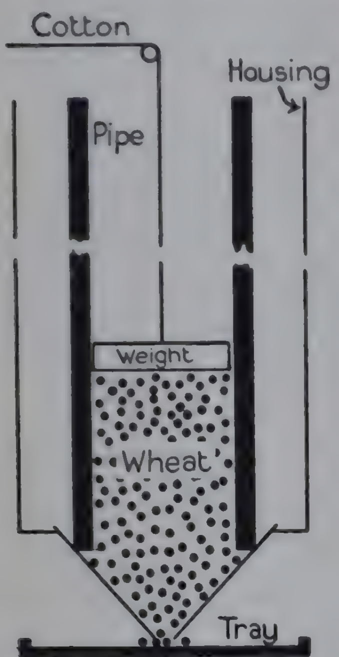

FIG. 1. Hopper of food-recording machine: rats remove wheat from the tray, and as the weight sinks in the pipe (different sizes may be used), a strip of smoked paper, attached by a thread, is drawn along under a recording clock. The tray is 14 in. square and the total height of the apparatus is 26 in. (Scale diagrams in Chitty and Shorten, 1946.)

Marking. A depilatory was used so that individuals could be recognized without re-trapping (Chitty and Shorten, 1946). The bared flesh occasionally had a piebald appearance, no doubt due to the pigmentation of the skin which precedes active hair growth; Dieke (1947) gives some useful references. Caged rats were marked by clipping patches of fur or by punching holes in the ears. An apparatus for holding wild rats during such operations is described by Emlen (1944).

Watching. Much information about behaviour can be gained by watching populations which are normally active by day or are attracted to baits; they will also feed readily under artificial illumination after the first few nights.

Automatic feeding records. An apparatus which records the amount of wheat taken by an entire population throughout the 24 hours is described by Chitty and Shorten (1946) and the principle is also illustrated in Fig. 1. Readings are obtained in terms of the displacement of a sheet of paper which is moved by a weight resting on a column of wheat in a hopper. The apparatus can be used in any rat colony, but most of our experiments were done at one place (Fig. 2 and Plate 4*a*).

Experimental colony. Some of our most important information about wild rats' behaviour has come from observation at a pig farm (Plate 4*a*) where the occupier agreed not to interfere with the growth of the rat population. Frequent references will be found to this experimental colony of wild rats and we acknowledge the very kind co-operation always received from Mr. Charles Giles. When observations began in 1941 the rats were living on garbage and other pig food. During the following months their numbers increased as a result of the experimental baiting, and by cutting off this extra supply, a state of hunger could be produced similar to that believed to occur in certain other habitats. The extent to which the rats depended on our baits was not understood for some time and in any future scheme there should be an alternative, preferably controlled, source of food in addition to the experimental baiting points.

Consideration should also be given to establishing wild experimental colonies from cage-reared stock of mixed brown and albino parentage (see Thompson, 1947, for examples of these crosses). White markings often occur in wild populations and many individuals have distinctive patterns (Doty, 1945; Anon., 1948). Alternatively a wild white strain could be used in conjunction with colour marking or perhaps internal staining of the growing hair by the injection method of Haddow *et al.* (1945). Minckler and Pease (1938) have observed a large albino population living in the wild.

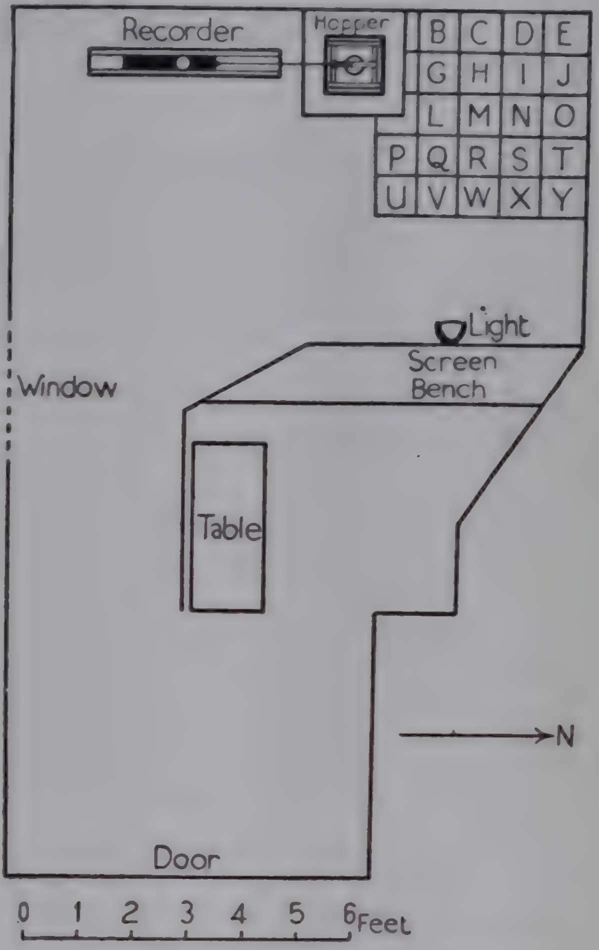

FIG. 2. Plan of shed at the experimental colony. The rats' main entrance hole was in the corner of square *E*.

Outdoor cages. Supplementary information was obtained from rats kept in large outdoor cages of wire netting. At first they were open to the weather, and mounds of earth were provided to simulate natural conditions; but because it was difficult to recover the corpses, the system was later modified so that the rats lived in a large nest box (Chitty, 1947). Floor space was 50 sq. ft. in each of four adjacent cages and 60 sq. ft. in another. The four main cages were protected from rain and to some extent from the sun. Rats could be watched with great convenience from our laboratory windows.

Tests of poisons were often carried out as a preliminary to the more expensive field trials, but it was a fault of the earlier work that no alternative food was provided during poisoning. Later on the rats always had plenty of plain wheat, and prebaiting and poisoning were carried out in the holes in the earth or at the exit from the nest box. These cage trials were of very little value in showing what sort of efficiency might be expected from a poison bait in the wild as the caged rats proved so much the easier to kill. A bait that fails to kill captive rats obviously need not be tested in the field, but this seldom happened and, in fact, when poison-shy survivors were needed a special technique had to be developed (§ 17 (*c*)). These experiments on shyness were far more profitable, and the method can be recommended for its economy and apparently close approximation to results obtained at great labour in the field.

Supplies of rats were obtained locally by live trapping, but this proved too laborious once the large infestations had been eliminated. Rats were also caught by hand at threshing. They travelled well in a dustbin with a wire netting cover and a supply of damp bait (Thompson, 1947). As this method of supply was seasonal and laborious, it proved profitable to supplement our stocks through breeding (§ 7).

(ii) *Research methods in Baltimore*

A major development in rodent control resulted from one of those accidents which, as Baker (1942) stresses, are so important a feature in research. A few crystals of phenyl thiocarbamide (phenyl thiourea) were placed on the tongues of six rats as part of a study by Dr. Richter and his colleagues on self-selection of diet. This substance, which was not known to be toxic, killed all six rats. (It has a bitter taste to some humans, but not to others whose inability to detect it is inherited as a recessive character.)

Developments from this discovery, described by Richter (1945) and Ward (1946), included the production of Antu as a rodenticide (Ch. 2), a mass of laboratory work on its properties, and the growth of a new permanent organization, the Rodent Ecology Unit. This group was originally formed to test the efficiency of Antu in the field and was first headed by Dr. John T. Emlen, Jr., and later by Dr. David E. Davis.

The main environmental unit studied in Baltimore was the city block of 40–50 houses which supported fairly discrete units of rat populations. The census method consisted of mapping all fresh evidence of rat occupation, classifying the result as so many infestation units, and then converting to an estimate of numbers (Emlen, Stokes, and Winsor, 1948). The latter is a subjective procedure but backed by considerable experience, chiefly in trapping out the rat populations of eighty-nine blocks.

When used for estimating comparative abundance just before and after control treatments (Emlen and Stokes, 1947) there is no reason to suspect any inherent bias in the census-survey method, and the precision of the estimates of actual numbers is not important. Similarly, there is unlikely to have been any systematic bias, e.g. according to population size, in the estimates of rate of recovery of reduced populations (Emlen, Stokes, and Winsor, 1948). Here, however, the amount of variation in the estimates is rather more important since an attempt was being made to establish the nature of the population growth pattern; and while the data show broad differences in the recovery rate it is questionable whether they are adequate to establish a logistic form of increase. These authors, in fact, are careful to present their case as a hypothesis; but in two subsequent papers a less cautious view is taken (Davis, Emlen, and Stokes, 1948; Davis, 1948).

Emlen, Stokes, and Davis (1949) discuss the various alternative methods of census which they tested and rejected. They also provide evidence about the way their own estimates check between workers and with known reductions in rat numbers after trapping (but they do not give the evidence for their estimates of total population). Prebaiting was used only as an accessory method of census and we ourselves would not have relied upon it in these populous environments.

The purpose of this brief review is to show that we are in essential agreement with the field methods of this group of workers and that the reasons for our differences in practical recommendations can be discussed without further reference to differences in methods of research.

(*d*) **Conclusions**

The efficiency of rat control must be measured by comparing the abundance of a population before and after treatment. Secondly, it is preferable to know, at least roughly, the relative sizes of different infestations, for the significance of results must depend on the size of the sample. Thirdly, some idea is needed of the actual number of animals.

A modification of one or more recognized methods of census may prove satisfactory for certain populations, especially if study is limited to one type of habitat. In such cases carefully standardized methods may yield information about the relative or even absolute abundance of different populations. The work carried out in the city blocks of Baltimore is a good example of this approach. Since our own work made use of infestations of all kinds, at all times of year and in some other countries, none of the existing methods seemed adaptable to the great diversity of habitat. Thus a sight count which might prove satisfactory on a dump would be impossible in a bank of tall weeds; tracks in a library are less obvious than on the farm, and the use of holes, droppings, live trapping, &c., is not equally effective in back alleys, flour mills, sewers, or the camel lines outside Khartoum.

The census baiting method, shortly to be described, is satisfactory for comparing original and residual populations in a great variety of habitats, and is generally adaptable to other requirements. The limitations and errors of the method have not proved as great as in others, most of which involve much more work (Plate 2).

4. CENSUS AND POISON BAITING AT THE SAME SITES

The amount of food eaten per day is the property of the rat population from which we found it most useful to obtain an index of abundance. This property has the added advantage of being the one of most practical concern in the case of a food pest. We were at first unaware of the extensive use of the prebaiting method already made by Doty (1938) both for research and control in Hawaii. This work is reviewed later after the early development of our own studies has been presented together with the different conclusions we reached.

(*a*) History and terminology

(i) *Surplus prebaiting*

In December 1940 an opportunity arose for studying the amount of food eaten by a colony of wild rats. Two hen houses were found to be badly infested by rats feeding on a dry cereal mixture constantly exposed in the troughs. Apart from a field of kale there was no obvious supply of any other kind of food. Certain changes were made to facilitate weighing the meal, and after an initial period of adjustment the rats took over 3 kg. per night (120 fowls were eating only twice this much). Arsenious oxide, 'arsenic', was added on the fifth night, after which a single night's observation showed a heavy reduction in feeding on plain meal.

A single food supply which can be weighed in this manner is comparatively rare; but it was thought that if plenty of bait were offered to other colonies they might be drawn away from their normal food so that their feeding capacity could be measured. If this were generally possible we would be able to compare the relative size of different colonies; and, if we knew the average amount of food taken per rat, actual numbers would also be known (Chitty, 1942). Even if it could not be assumed that the full feeding capacity was being measured, surplus baiting should give the relative population size before and after poisoning.

Many experiments in the first part of 1941 showed the remarkable way in which brown rats could be attracted to a new source of food. An amount expected to be larger than the rats could eat was placed one day and weighed the next. In most cases whole wheat was used, protected where necessary from the weather and other animals (Ch. 3) in a 'protected poison point' (P3) or in 'pipes': mole drain pipes 15 in. long, 3 in. internal diameter, blocked at one end. Other containers such as tin cans and boxes were used; or in buildings the bait was laid on any smooth surface ('surface baiting').

There was usually a 'lag' of several days before the start of any feeding in containers because the rats avoided these new objects ('new object reaction', Ch. 5). To allow for this, daily weighings were seldom started until 14 days after placing and baiting the containers. It was otherwise necessary to waste many visits to a locality before any of the containers were entered at all; and since the lag differed between containers, baiting was necessarily prolonged until observations on the last of them were complete. Even after allowing for lag there was generally a period of 'build-up', characteristically followed by a 'peak' or highest take and then a fairly steady level of feeding.

After this steady level had been reached, the population could be poisoned either with 5% fine arsenic added to the wheat or by changing to another food or 'base' for a few days and adding arsenic, barium carbonate, or red squill powder. After the first day negligible amounts of poison bait were taken, and in most experiments a 'postbait' of plain wheat was offered immediately after the first day's poisoning to find out the reduction in take. As this postbait might also serve as a prebait for another poisoning, consecutive periods of baiting with a surplus of plain food were generally referred to as census 1, 2, 3, &c.

(ii) *Token prebaiting*

From July 1941 experiments were started on the effect of reducing the prebait to amounts which could be entirely consumed in 24 hours, a system called 'token' prebaiting. During the next month Elton noticed a remarkable difference in behaviour between rats given token and surplus baits. Forty pipes on a rubbish dump were each baited with 20 g. wheat in the late afternoon, 3–4 hours before sunset, and by the fourth day rats came out to feed as soon as the pipes had been baited. Five days later twenty-two individuals at least were identified at one spot. Surplus bait was then given, and at the same time 2 days later one or two rats only were seen. The same behaviour was noticed by Venables in a similar experiment carried out soon afterwards at another rubbish dump. Rats appeared soon after the pipes were token baited and again hardly any were seen after the first night of surplus baiting. The take of surplus bait immediately after token baiting was larger than on the next 2 days: 10,745, 10,375, 8,365 g. The greater eagerness shown by token-baited rats suggested that this system of prebaiting might be more efficient, besides more economical, than the use of surplus amounts.

On small populations successful control was achieved by token baiting in P3s or pipes; but results with larger colonies were less satisfactory, perhaps because insufficient containers had been placed (§ 14 (*b*)). In May 1942, before any further work was done on this matter, the 'hole baiting' system was introduced (Ch. 10). Token quantities of prebait and adequate amounts of poison bait were placed well down all burrows, under cover or on the surface if safety permitted. This so simplified large-scale rat control that containers were seldom afterwards used except in census work and for controlling small colonies. Also, it has since been realized, there are a number of objections to the system of both census baiting and poisoning in the same container.

(*b*) **Errors in census**

(i) *Sampling errors*

In a few of our early tests some relatively high takes of wheat after a poisoning suggested that the baits were almost useless; but, evidence from trace patches, lack of bait shyness, &c., suggested that different rats had moved into the vacant niches and were taking the postbaits. Since the object of these experiments had been to measure the efficiency of the poison baits they failed to give a reliable answer and it was only safe to conclude that the bait or its distribution or both had been faulty.

Incomplete sampling can probably be avoided, given enough knowledge about movements and with adequate spacing of points; but in a large infestation it is easy to leave some animals untreated. If they move around later on and find the census points the killing powers of the bait will be underestimated. By contrast, the estimated proportion of rats killed in the area as a whole will vary in an indeterminate manner according to the amount of movement and extent of the area sampled. Should there be no immediate redistribution of untreated rats the killing powers of the bait will indeed be found, but the estimated reduction of the population will be favourably biased through measuring the effects of treatment only at those places where it is applied.

These arguments are summed up in Fig. 3 where it is supposed that there was an original population of sixty-four rats and that the baits killed all which took them. It will be seen that whether or not the rats extend their ranges, this system will give least information. In measuring the clearance of rats from a sewer system (Ch. 12) the danger of biased sampling is particularly acute; for, except in large sewers, rats cannot be census baited and poisoned except at the manholes. Little is known about the distribution elsewhere; but in addition to the main channels there are fairly extensive drains under neighbouring property, and rats may also have access to these premises and to the soil. There is evidence that not all rats move freely between adjacent manholes (Ch. 12, Fig. 2) and it is at least possible that some may be confined to the areas between them. Rats which have not been baited at the manholes may therefore take time to be attracted to the postbaits, and evidence about clearance of the system as a whole may not be valid unless confirmed after some weeks.

(ii) *Place shyness*

Tests in containers are further complicated by the possibility that some survivors may avoid them and thus not take the postbait. Our evidence about this is fragmentary; but certain observations show that there may be a tendency to avoid the site of an unpleasant experience such as being poisoned or trapped. In the majority of cases this tendency is no doubt quickly overcome and is not believed to have been an important error in our own tests, where poison was seldom exposed for more than one day.

The following examples concern rather small populations and run counter to our general experience. (We have no evidence (§ 8 (*i*)) that the habitat itself is ever deserted as a result of an unsuccessful poisoning.) (*a*) In an outdoor cage test (Fig. 17) 1% arsenic in sugar-meal was left in a P3 for 9 days. After a high take the first day there were visits until the

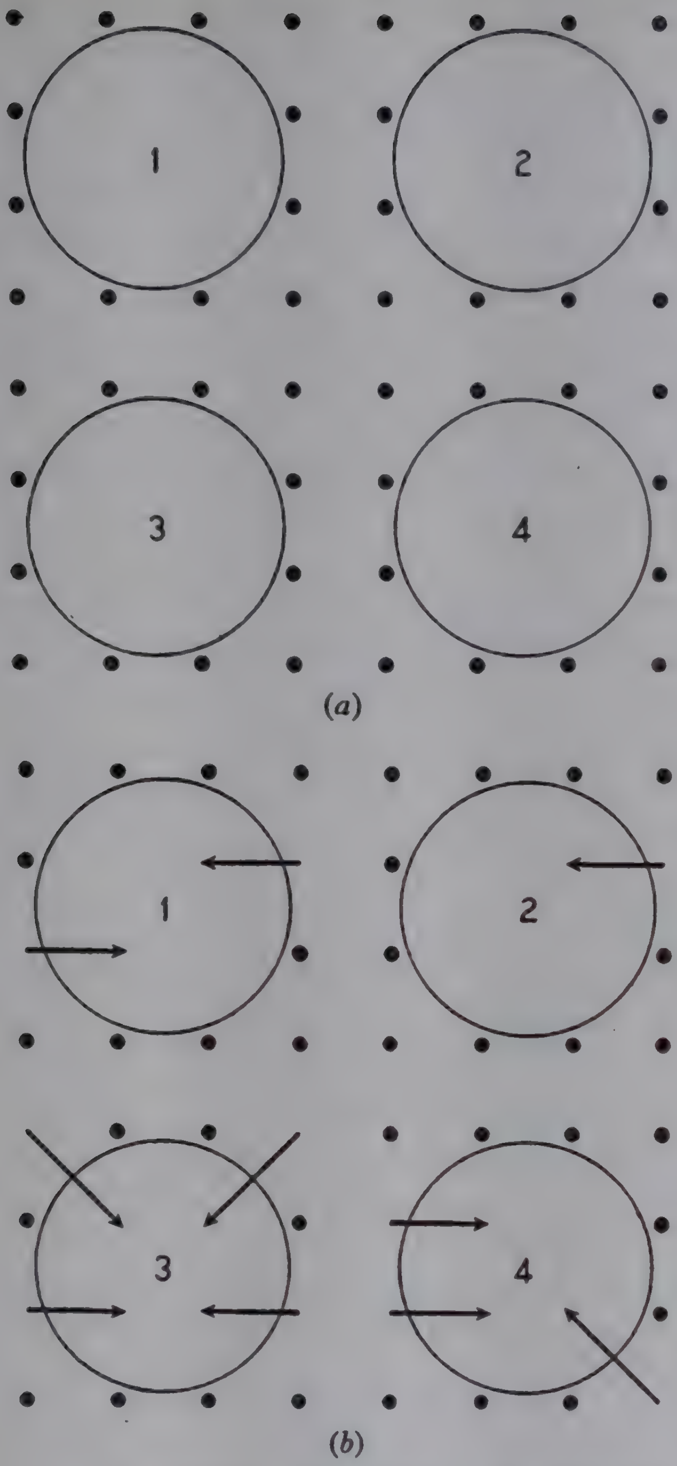

FIG. 3. Schematic illustration of bias introduced by poisoning and postbaiting at the same sites only. Each spot represents the location of one rat. Nos. 1–4 are census and poisoning points at which it is assumed that the nearest four rats are killed by poison. (*a*) If these niches are not filled by other rats the efficiency of the poison will be correctly estimated, but not that of the clearance of the whole area. (*b*) If certain rats move in to take the postbaits the efficiency of the poison will be underestimated; that of the clearance, 25% in this example, will still be unknown. (According to the various amounts of infiltration the estimates of success at the four points are (1) 50%, (2) 75%, (3) 0%, (4) 25%.)

seventh day, after which the P3 was not entered. Wheat was substituted on the tenth day, but none was taken for 4 days and rather small amounts for a further two. (*b*) In the field a P3 containing wheat was similarly deserted after poisoned and later plain flour had been poorly taken for several days. (*c*) In another field test, crude arsenic in kibbled wheat was exposed for 1 day only and the amount taken was too small to be detected; but no wheat was taken from the P3 during a 4-day postbait. (*d*) In five other instances a low postbait was probably explained by at least a temporary avoidance of the container. In each of these cases there was independent evidence to throw doubt on the apparent success. (*e*) See p. 298, rat no. 7.

Thus by census baiting and poisoning in the same containers the true efficiency of a poison bait may be concealed for one of two opposite reasons: its apparent success may be because of place-shyness or its apparent failure may be because rats not previously census baited and poisoned have occupied the vacant territory. However, it seems most unlikely that avoidance of the site of a poisoning occurred very often or lasted for long. The examples quoted, all on small populations, are the only ones for which there is any such evidence, and it was a regular procedure to make an independent check for signs of survivors.

(iii) *Bait base shyness*

In the examples quoted, poisoning had been done with a different base from that used for postbait; but where the same base is used for both, survivors may refuse the postbait entirely or in part, or return to it only gradually (§ 17). With some bases, notably whole wheat, there may be no shyness at all, but it is particularly marked after certain poison baits, and must result in an over-estimate of the success achieved. An example was provided in a test in which kibbled wheat was used throughout both for census and poisoning. 'Success' based on the postbait during the first week after poisoning was almost 90%; but after this the takes of kibbled wheat gradually rose to the original level of about 800 g. and the rats refused a second dose of poison.

(*c*) **Research in Hawaii**

The most outstanding contribution to rat control before the Second World War was that of Doty (1938). This paper describes not only an objective way of assessing field results but a method of control evidently superior to those previously used. Covered feeding stations were established throughout the cane fields and rolled oats was used for surplus prebaiting and poisoning with thallium sulphate at 0·4%. Poisoning was followed by baiting with plain rolled oats again; but the chief claim to success made in this paper was the very large amounts of poison bait eaten and the probable minimum number of rats killed. There was also much supplementary evidence of reduction in numbers of rats, confirmed later on (1944) by details about the fall in numbers trapped each year. A further paper (1945) repeated much of the original information and added a great deal more, leaving little doubt about the general effectiveness of the practical work. However, certain questions need to be raised.

In the 1945 paper evidence about the effectiveness of the trials depends mainly on one of two methods: a comparison of the take of plain oats before and after poisoning at the same stations, and deductions from the amounts of poison bait taken on successive occasions. It will be realized from the discussion just given that our own experience does not permit us, without further inquiry, to accept either of these methods as providing valid evidence. Nevertheless, the data suggest that this first method may have been reasonably sound under the specific circumstances but that the second was faulty.

In the two original trials (requoted in 1945, pp. 145–52) poison bait was exposed for 6–7 days, and in the next 3–4 days the take of plain oats was reduced to 1% of its former level. The doubt is whether a 99% reduction in numbers may be inferred for the whole experimental area or whether this figure is only apparent because of base-shyness, place-shyness, or the presence of unbaited rats between stations. Doty, indeed, considers the possibility of base-shyness after a trial in which barium carbonate and thallium sulphate were both rather poorly taken (1945, p. 180) but in which the subsequent take of plain rolled oats was reduced to 1·2% of the original. However, this test may, in fact, have been successful: rats neither avoided the site nor the base after tests in which inefficient baits of red squill or phosphorus mixed with oats had been exposed for 3–4 days. These rats also took normally to oats and thallium sulphate. Similarly, rats given a choice of oats with and without thallium sulphate were able to distinguish between them and continued visiting the stations (1945, p. 160). Doty also examined the possibility that nuclei of rats might remain in the areas between stations, which in one test were spaced rather more than the standard distance apart. Even under these conditions comparatively little bait was taken from newly interspersed stations (1945, pp. 147–8).

Although no final appraisal can be given, these data suggest that an experimental procedure that had to be discarded in our own work may have introduced little or no bias under other conditions. It must be emphasized that two other species of rats (*R. hawaiiensis* and *R. rattus*) were present besides the brown rat and that even for the latter we have no sure basis for predicting behaviour towards repeated doses of poison.

Less certainty attaches to Doty's interpretation of the results of repeated poison treatments. The efficiency of poisoning was compared after one or two periods of prebaiting or none at all, each being followed by a standard prebaiting treatment (1945, p. 152). The assumption is made that irrespective of the length of the prebaiting period (or lack of any at all) the quantity of poison bait consumed is proportional to the number of rats killed. Yet elsewhere (Doty, 1945, p. 116) it is stated that populations which have been partly or not at all prebaited may eat less poison than those that have. However, even if the actual figures are questionable, it seems clear that direct poisoning and incomplete prebaiting were failures. It is shown (1945, p. 161) that some experienced rats took the same poison bait a second time. This being so it is difficult to accept Doty's explanation (p. 156) that 'The percentage of poisoned oats eaten during the second period represents the amount of reinfestation . . .' rather than a partial failure. Thus in nine of the fifteen trials summarized in his table 13 the average takes of first and second poison baits were respectively 76·4% and 15·6% of the original take of plain bait. This is interpreted to show a reinfestation of about 20%; but the possibility that survivors were responsible for at least part of the take cannot be dismissed. (The same argument applied to one of our tests, for example, indicates a reinfestation of 65% instead of the survival of most of the original rats.)

It is fair comment, I believe, that this and certain other conclusions based on takes of poison (e.g. 1945, p. 161) must be accepted with reserve until more precise evidence is given about behaviour towards second treatments with the same poison bait. This is a complicated subject of special interest, particularly with a fairly tasteless poison such as thallium sulphate in a very low concentration.

(*d*) Conclusions

Wild brown rats may be attracted from their normal food to suitably placed baiting stations. Prebaits may be given in excess of the daily requirements or be small enough to add little to the normal food resources. Surplus prebaiting followed by poisoning was used successfully in our first experiments and is current practice in Hawaii. An advantage of this system is that a surplus may be provided for several days, thus cutting down the number of visits by the operator. However, not only does a great deal of prebait have to be carried, but small daily amounts may be more effective in bringing about rapid feeding. Under conditions in Great Britain, therefore, surplus prebaiting before poisoning is not an economical method of control or the most efficient.

As a research method the amount of plain bait taken before and after poisoning at the same sites may be used to find the proportion of the population killed by poison; but valid estimates of efficiency are not always obtainable, for there are a number of ways in which bias may be introduced. The chief difficulty is to make sure that no rats are missed through incorrect placing of the stations within the occupied areas. If this fault occurs there will be pockets of untreated animals and no measure of the clearance of the area will be obtained. Information may, with luck, be gained about the killing powers of the poison bait, but in other cases the results may be misinterpreted because of movements by untreated rats into the areas around the baiting stations. While these errors are likely to occur only in the more complex infestations other faults may be introduced if there is any shyness on the part of the survivors towards the postbait or site of the poisoning. The possibility of serious error can be minimized by additional checks such as using trace patches and searching for droppings.

5. INDEPENDENT CENSUS AND POISON BAITING

Although nearly all our early field work consisted of census baiting and poisoning in the same containers, we are not obliged to rely upon many of these results. In our later system we no longer used containers for poisoning but only for census baiting, which now became an entirely separate operation from prebaiting the rats with the food or at the sites to be treated with poison. Prebaiting for this latter purpose was now carried out at a large number of independent points (Fig. 16; Plate 3) and nearly always with a different base from the wheat used for the census.

(*a*) **Characteristics of the data**

A series of Rat Control Tests (R.C.T.) carried out by Middleton and Freeman (Table 4) illustrates this method and introduces various problems of interpreting the data. This series involved no prebaiting in the burrows since it was to test the efficiency of direct poisoning with zinc phosphide; the research procedure, however, was the same except for this omission. All the sites were within a few miles of each other on the borders of Norfolk and the Isle of Ely, and though varying considerably in environment, were entirely rural infestations. All except one (R.C.T. 818) were first visited on 5–8 February 1945, when pipes were placed and filled with about 200 g. wheat each. After 2 weeks the sites were revisited and census baiting began at once or after preliminary token baiting, and was continued for several days. The wheat take was measured daily except for the first 2 days of census. There was then a gap of 3 days, during which the populations are presumed to have returned to their normal feeding habits. Poisoning was carried out by Middleton on the hole baiting system between 11 a.m. and 4.30 p.m. G.M.T. on one of the days between 27 February and 4 March. On the following day a search was made for corpses and for uneaten poison baits in places likely to become accessible to other animals. The pipes were then refilled with wheat, which was again measured daily except for the first 2 days in some cases.

Except in R.C.T. 818, where pipes were not used, the majority of peaks in the first census occurred on the fourth or fifth day of surplus baiting. In the postbait the highest takes mostly occurred on the second or third day but four came on the fifth. In two cases (R.C.T. 809, 823) the increase in take was only 10 g. over the earlier takes and may be neglected. In R.C.T. 808 the fluctuation from 0 to 140 g. is more serious and a longer postbait would have been desirable. R.C.T. 814 is exceptional in showing a

Table 4. *Take of wheat (g.) before and after direct poisoning*

R.C.T.	Census 1 Days from poisoning									Census 2 Days from poisoning					No. of census points	No. of poisoning points	No. of corpses picked up
	−12	−11	−10	−9	−8	−7	−6	−5	−4	1	2	3	4	5			
								(a) Rush + 5% zinc phosphide									
808	..	..	..	..	405 - - -	405	480	**550**	540	60 - - -	60	0	60	**140**	2	18	1
809	..	..	..	..	55 - - -	55	**250**	230	210	10 - - -	10	0	0	**20**	1	8	0
810	..	..	..	..	455 - - -	455	460	**920**	880	290	370	200	70	**320**	3	32	2
811	..	..	..	..	145 - - -	145	260	**430**	410	50	190	90	90	**100**	2	16	3
812	..	..	..	..	260 - - -	260	360	**410**	410	0	50	0	0	0	2	18	0
813	..	*	..	330	430	360	630	**690**	680	110	150	340	**380**	370	4	59	11
814	*	..	1,005 - - -	1,005^{+}	830	960	**1,200**	1,160	1,180	710^{+}	**710**	560	660	(1,430)	6	86	7
815	..	..	..	..	590 - - -	590	690	**920**	850	520	530	**700**	490	550	6	45	2
816	..	..	*	210 - - -	210	847	960	**1,020**	930	70	100	120	**170**	50	4	148	1
817	..	..	..	355 - - -	355	600	900	**940**	940	220	330	**350**	300	320	3	92	4
								(b) Sugar-meal + 5% zinc phosphide									
818	..	..	..	..	c. 10	870	**770**	760	730	0	0	0	0	0	2	32	5
819	..	..	..	400 - - -	400	860	**1,020**	980	960	470^{+}	470	320	**420**	420	4	120	3
820	..	..	..	..	325 - - -	325	500	**620**	560	15 - - -	15	**30**	0	0	3	20	1
821	..	..	..	..	135 - - -	135	250	**290**	260	75 - - -	75	120	**130**	120	2	22	0
822	..	..	..	..	180 - - -	180	280	**320**	300	30	50	**60**	60	60	1	14	1
823	..	..	..	..	250 - - -	250^{+}	380	**430**	410	20	130	0	70	**140**	1	12	1
824	..	..	*	..	*	..	420	**510**	480	0	100	60	**90**	80	2	22	1
825	..	..	*	..	120 - - -	120	210	**280**	240	0	0	**40**	30	40	3	18	0
826	..	..	*	..	195 - - -	195	260	**340**	320	140	160	**160**	160	160	2	8	2
827	..	*	..	80 - - -	80	110	200	**240**	240	160	200	**150**	120	130	2	9	0
828	..	*	..	510 - - -	510^{+}	450	640	**650**	620	0	0	0	0	0	5	21	0
829	..	..	*	435 - - -	435	650	750	**770**	760	220	210	**260**	160	210	4	52	7

* Token bait given. Hyphens show average takes. $^{+}$ Indicates no surplus at some or all points.

Heavy type shows peak takes as defined in the text.

doubled take on the fifth day to a figure higher than any in the first census. In this case it is fairly certain that immigration had occurred from a nearby habitat which had been set on fire the previous day.

Thus within 5 days from the start of both census baitings the feeding rates of these populations had mostly reached some sort of equilibrium; but before we can make use of these apparent equilibria we have to examine the figures more closely. For example, the later takes in the first censuses suggest that wheat formed a fairly constant proportion of the total diet of the population. No assumptions are involved at this stage about the amount of normal food eaten as well, but proof is needed that wheat consumption bears a relation to some property of the population such as numbers or biomass, i.e. it must be shown that the amount taken was not dictated merely by the difficulty of removing wheat from the containers and was not due to hoarding or other unknown behaviour. In the postbait to R.C.T. 814, as already stated, equilibrium was not reached because of immigration; and we must also consider the general question of whether poisoning produces any change, apart from reduction in numbers, which makes the second equilibrium no longer comparable with the first.

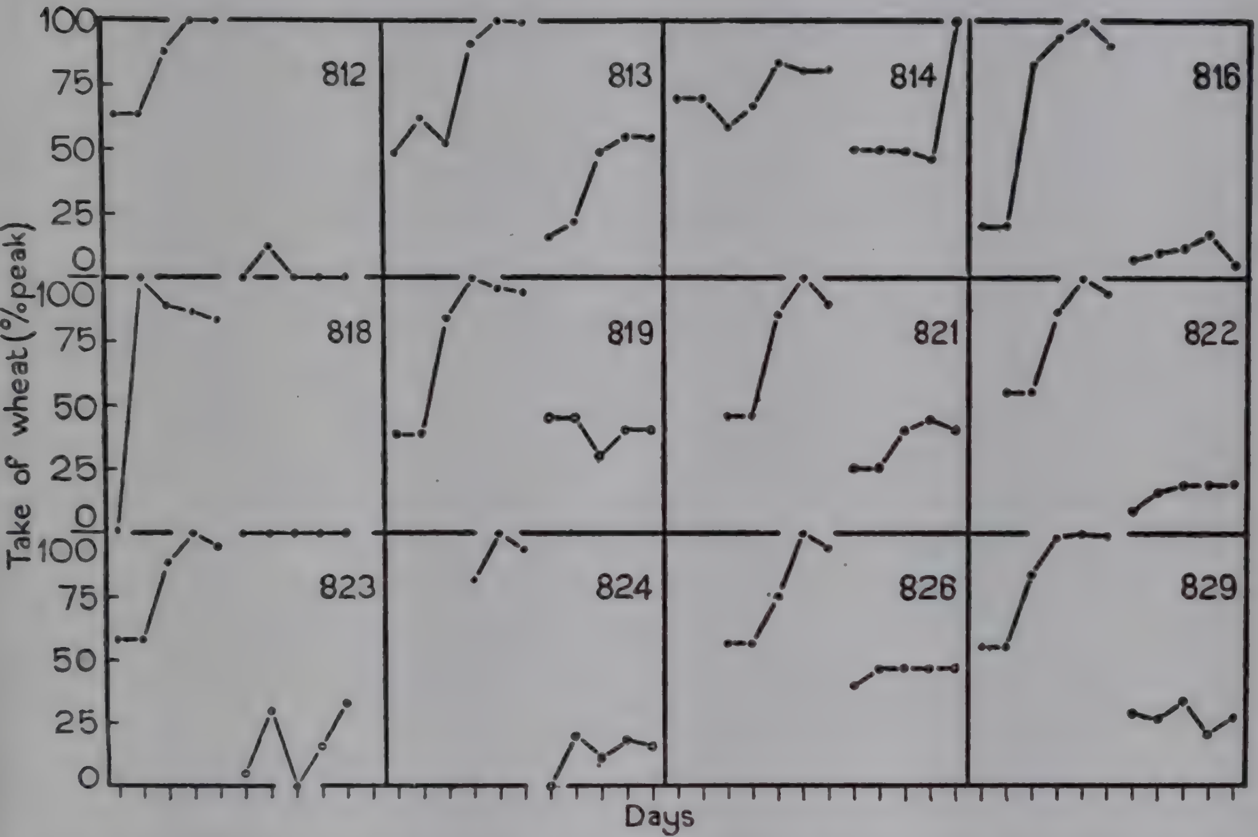

FIG. 4. Selected examples of the results of direct poisoning as estimated by census baiting. Daily takes of wheat are plotted as per cent. peak take in census 1. The postbait (= census 2) starts on the day after poisoning, i.e. 5 days after the end of census 1. Numbers are those of R.C.T. in Table 4.

R.C.T. 818 is the other instance in which there is doubt about the kind of equilibrium established. Here the first day's take was negligible and after the second day's peak there was a falling take. The events on the first day are explained by the fact that wheat was offered on two newly placed slates of which the rats were at first shy; but a falling off in take is a peculiarity which often has to be taken into account.

(*b*) Expressing the results

In order to answer the questions raised by these figures it is necessary to consider some fairly extensive data from other tests. However, it will be shown that objections to the wheat take as a measure of the success of poisoning can usually be met; and before proceeding we may consider the best way of dealing with the figures once their validity is granted.

Consider first R.C.T. 814. Here, after the first 4 days, the rats took a very constant amount of wheat: 1,200, 1,160, 1,180 g. and the average of these last 3 days could be taken. In R.C.T. 810, 811, and 815, however, rats had obviously not adapted themselves to the wheat before the last 2 days of census, and the most representative average should probably not include any earlier takes. Similarly, if R.C.T. 827–9 had been limited to 5 instead of 6 days the averages of the last 3 days would have been lower because of the inclusion of takes during the build-up phase. An average may therefore have one of two faults: it may lower the estimate for the phase of equilibrium if a decision is made to average an arbitrary number of days' takes; but if each case is decided on its merits there will be difficulty in arriving at an objective figure. For example, in R.C.T. 826 the last 2 days might be selected by one worker while another might decide that there was no significant difference between the last three.

The postbait takes in this series show rather considerable variations and some possible explanations are advanced at this stage before being discussed in detail. There are some indications of low takes at first, perhaps due to illness (R.C.T. 813, 825), and of small takes followed by none at all (R.C.T. 812, 820) perhaps due to delayed death. There are also certain takes during the first 3 days (R.C.T. 811, 815, 827) that are a good deal higher than any which followed. These might well represent excessive feeding by rats which were hungry as a result of reduced feeding during illness. Other fluctuating takes (R.C.T. 808, 810, 823) might be caused by changes in the natural food or by disturbance.

It seems best to reject at least the first 2 days' postbait and to average the remaining three. However, such an average may not be representative where an unusually low take must be included and it might be advisable to reject altogether a comparison based on averages. Instead let us consider the peak takes. In the first census baitings, few peak takes are much higher than other takes during the phase of equilibrium. R.C.T. 818 is a slight exception with its falling take; but certain other tests might have shown much higher initial takes had they not lacked a surplus for the first 2 days. As will be shown later it is a good general principle to disregard at least those peaks occurring within the first 2 days. The same rule can be applied to the postbaits, where again high initial takes may be unrepresentative (§§ 6, 7). From the third to the fifth days of postbaiting some of the peak takes are well above the next highest takes (R.C.T. 808, 810, 814, 815, 823), though not always above those on the first 2 days.

Objections to using peak takes are that they may be only one of many chance variations; they may be affected by hunger, especially because of a low take the previous day; they may be caused by hoarding, immigration, or error in measurement, against all of which possibilities an average might prove better. The twenty-two tests in Table 4 have therefore been compared on both systems in Table 5, peaks and averages being taken from the last 3 days of census 1 and third to fifth days of census 2 (second to fourth days in R.C.T. 814). Agreement is sufficiently close, the only tests which differ by >5% being R.C.T. 823 (67 and 83%), 829 (66 and 72%), 808 (75 and 87%). The mean of the percentages for the sugar-meal series is 73–76%, if each test is given equal weight or 76–79% worked out from the pooled totals. For rusk the figures are 66–71% or 61–65%. Therefore if this series is representative either peaks or averages may evidently be used.

A further expression of results can be obtained by comparing the average for census 1 with the peak for census 2. This will mean that the original population tends to be underestimated in comparison with the residual population and hence that a lower limit to success can be stated. On the other hand, a comparison of peaks for census 1 with averages for census 2 will give the most generous estimate of success. Treating each test in this way and averaging the series as before, it is found that the highest and lowest estimates agree within 5–9%, a remarkably small gap. However, in any one test the true figure may lie anywhere between a rather wide range so that we must next consider what use is to be made of the data. Thus if we are interested in comparing the results of direct poisoning with those after prebating the range of error is very much less than the difference in results (§§ 11 and 12). On the other hand, the apparent advantage of sugar-meal over rusk as a base for direct poisoning is not established by these data.

Fortunately the differences in which we are primarily interested are of the first kind and are fairly easy to establish despite the uncertainty about the exact estimate of success. Since both methods of expression seem to be satisfactory there are good reasons for adopting peak takes in our comparison of population densities; the chief reasons are greater simplicity and the fact that many tests lack the correct data for any other standard treatments. We have not found it necessary or justifiable to apply statistical tests of significance.

Some further data on the different methods of calculating the results may conveniently be included to support the contention that peak takes are a sufficiently accurate index for present purposes. Table 5 (*c*) gives the results of a fully measured series of direct poisoning experiments with Antu. It may not be possible to make a comparison of efficiency between

TABLE 5. *Alternative ways of expressing census data*

R.C.T. No.	*Peak take of wheat (g.) in census 1*	*Food supply**	*Estimated kill (%)*			
			Lowest est.	*By peaks*	*By avs.*	*Highest est.*
(*a*) 5% *zinc phosphide in rusk*						
812	410	ab.	100	100	100	100
809	250	sc.	91	92	97	97
816	1,020	sc.	82	83	88	89
811	430	ab.	73	77	75	78
808	550	sc.	73	75	87	88
810	920	ab.	58	65	74	79
817	940	sc.	62	63	65	66
813	690	ab.	43	45	46	47
814	1,200	sc.	40	41	46	46
815	920	ab.	15	24	29	37
Average of the percentages . .			64	66	71	73
Average of pooled figures . . .			58	61	65	67
(*b*) 5% *zinc phosphide in sugar-meal*						
818	760	sc.	100	100	100	100
828	650	ab.	100	100	100	100
820	620	sc.	95	95	98	98
825	280	sc.	84	86	85	87
824	510	ab.	81	82	84	85
822	320	ab.	80	81	80	81
823	430	ab.	66	67	83	84
829	770	ab.	66	66	72	73
819	1,020	sc.	57	59	61	62
821	290	sc.	51	55	54	58
826	340	sc.	48	53	48	53
827	240	ab.	34	37	41	45
Average of the percentages . .			72	73	76	77
Average of pooled figures . . .			75	76	79	80
(*c*) 2% *Antu in rusk*						
1432	1,725	ab.	95	95	96	97
1435	4,565	sc.	85	87	86	87
1439	930	ab.	80	82	88	89
1433	190	sc.	65	66	74	75
1438	3,700	ab.	46	48	48	50
1436	1,040	?	46	47	49	49
1437	3,370	sc.	43	44	49	50
1434	405	ab.	28	32	31	36
1440	420	ab.	0	17	3	20
Average of the percentages . .			54	58	58	61
Average of pooled figures . . .			62	64	66	67

* ab. = abundant; sc. = scarce.

this poison and zinc phosphide, but the highly variable results with both should be contrasted with the consistently good results after prebaiting (Table 25). There can be little doubt that the census baiting method, despite certain difficulties now to be discussed, is adequate to deal with differences of this nature.

(*c*) Summary

Twenty-two different rat colonies were poisoned in the burrows and measurements were made of the daily amount of wheat taken from containers before and after treatment. The takes of wheat showed sufficient regularity to suggest that they were proportional to the size of the original and residual populations, but a number of assumptions were involved. Since the object of the tests was to obtain an unbiased estimate of the efficiency of the poisoning, two separate lines of inquiry were called for. The first of these was to check the accuracy of the assumptions, and certain special problems raised by the figures are quoted as examples of the arguments advanced in later sections. Secondly, an index had to be abstracted which would give a fair description of the results. It was concluded that a comparison of peak takes of wheat before and after the poisoning was satisfactory.

6. FACTORS AFFECTING THE TAKE

(*a*) Difference between take and intake

Intake of food is the amount actually ingested after correction for such errors as waste, loss and gain of moisture, &c. This precision cannot be matched in the field where the difference between the weight of bait set out one day and that remaining on the next is called the 'take'. In most experiments take and intake are perhaps about the same but there are a number of errors which make the one only an estimate of the other. The specialized meanings attached by McDougall (1944) to take and consumption are not followed here.

(i) *Recording the daily take*

Balance. A spring balance 1 kg. × 10 g. was used, accurate to 5–10 g. With a large number of small takes the error in the total may be fairly large; e.g. when measuring poison bait. In such cases several baits may be pooled, or weighing can be done in the laboratory.

Date. The date is sometimes a matter of confusion since bait is placed one day, e.g. 21 February, and taken up the next. Feeding covers two dates but is referred to that on which the take is known i.e. 22 February. Token baits, however, may all be eaten the same day, and the practical operator usually records the day the poison is laid. Our own convention is that, as the results are usually not observed until the following day, the second of two dates may again be used; but where it is necessary to refer to one specifically it is written 21/ or /22.

Time of visit in relation to feeding periods. Rats take most of their food during darkness (Chitty and Shorten, 1946), but in some habitats they may also eat during daylight. The census baits should therefore be visited at about the same time each day, though within normal working hours this error is not likely to be large. During the summer as much as 25% of the day's total may be taken between 6 p.m. and 10 p.m. so that evening visits should be avoided.

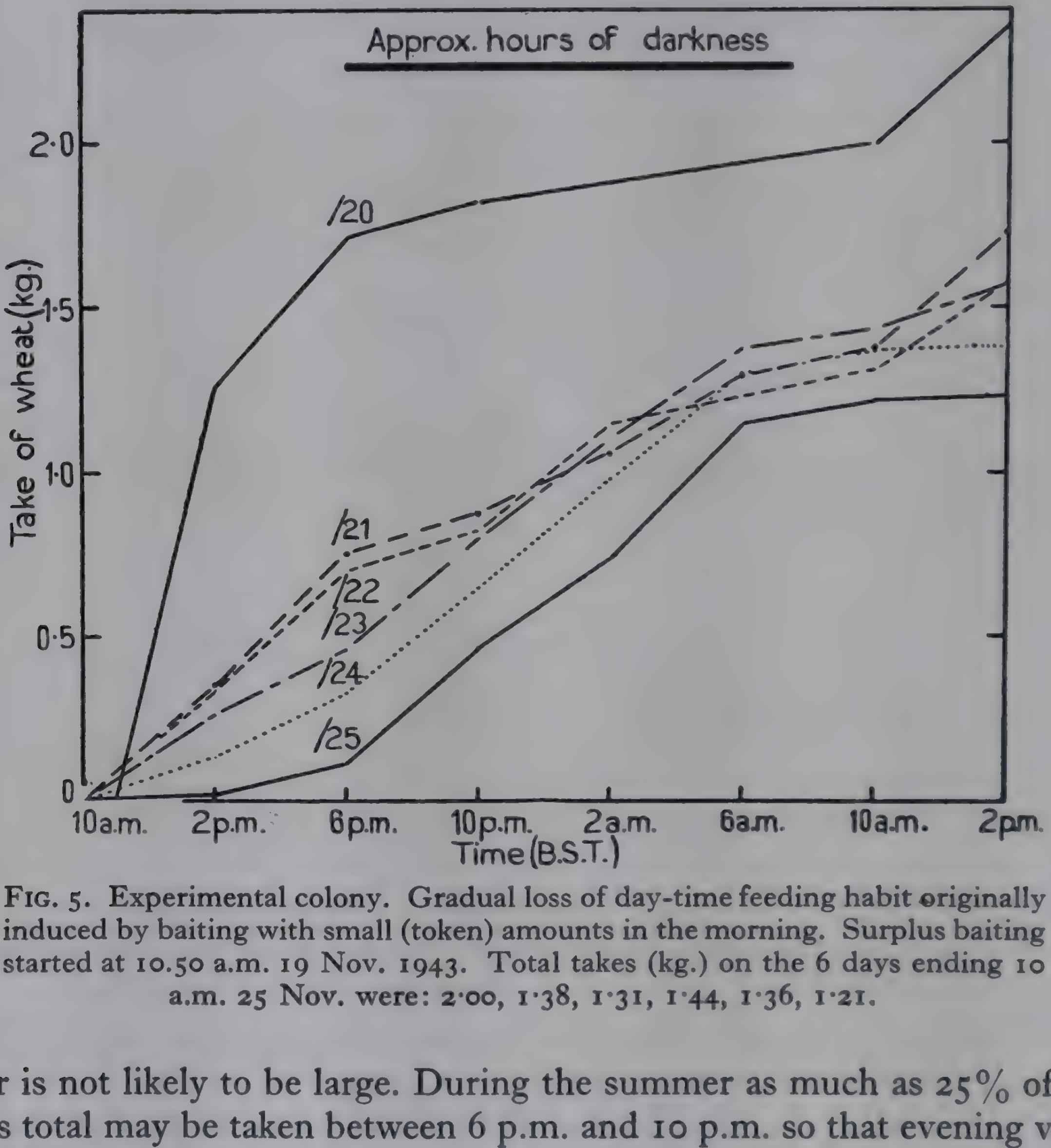

FIG. 5. Experimental colony. Gradual loss of day-time feeding habit originally induced by baiting with small (token) amounts in the morning. Surplus baiting started at 10.50 a.m. 19 Nov. 1943. Total takes (kg.) on the 6 days ending 10 a.m. 25 Nov. were: 2·00, 1·38, 1·31, 1·44, 1·36, 1·21.

There were two unusual cases of day-time feeding. Ranson found that fluctuations in take were correlated with the time he visited some bait points in a hen run. Low takes occurred when an afternoon visit was followed by one at 9 a.m. and high takes when a morning visit was followed by one next afternoon. These rats seem to have acquired the habit of feeding during the morning, and a 36-hour interval may thus have included two main feeding periods. Perhaps this habit had arisen because the rats had adapted their feeding to the times at which the fowls were fed: Cottam (1948) has observed brown rats which regularly appeared at feeding time at a fish hatchery. A similar change of habit was shown at the experimental colony for several days after the need for diurnal feeding had ended (Fig. 5). These rats had been given 500 g. token baits of wheat at 11 a.m. or noon during the previous 4 days. When a surplus was first restored they fed almost exclusively during the day and little that night (Fig. 6 *b*). The slow return to a more normal pattern may have been hastened on the

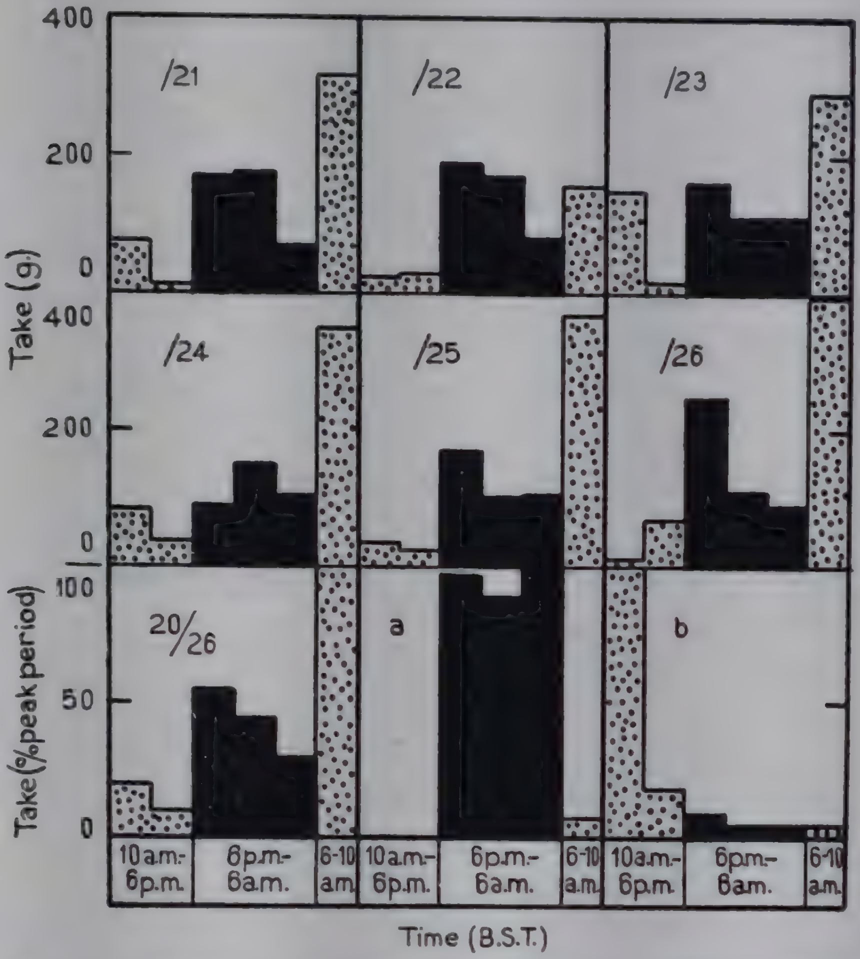

FIG. 6. Variations in feeding rate between periods. Black histograms show the amount of feeding in darkness; stippling shows daylight feeding. The two top rows show the atypical morning feeding between /21 and /26 Feb. 1943. In the bottom row the total for these days is compared with (*a*) the more usual pattern (Chitty and Shorten, 1946) and also with (*b*) the pattern on 19/20 Nov. 1943 (Fig. 5). (Figs. 5 and 6 are plotted on two different systems: Fig. 5 to emphasize the differences between days, Fig. 6 those between periods within days.)

sixth day (24/25 November) because the screen was left off the window. Normally the shed was fairly dark.

Some other rats spontaneously developed the habit of taking a high proportion of their food between 6 a.m. and 10 a.m. when normally most feeding was finished (Fig. 6). Here again differences in the time of weighing the residues might have introduced an appreciable error.

The residues. Rats may leave excreta in the bait. Faeces can be picked out, but some urine may be absorbed. Baits may get damp or dirty or buried in excavated earth. Various objects which may be stored in P3s (Ch. 3) are mostly large enough to be picked out easily. Wheat is slow to lose or take up atmospheric moisture, but wet baits lose water through evaporation (Ch. 14). Errors due to gains and losses in weight are mainly dependent upon the size of the residue and amount of scattering by the rats, so that the surplus should not be unnecessarily large in comparison with the expected take.

Other animals. Human interference can be serious and disturbances one day may affect the take for several days while the rats adjust themselves to the changes (e.g. Fig. 15). Interference from domestic animals must be guarded against before the experiment starts as serious delay may result from precautions taken during a test (Ch. 5).

Mice cannot be excluded from the baits, but except in corn-ricks their numbers have usually not been large where many rats were present. As mice may survive a poisoning that kills all the rats a slight error may be introduced into the postbait, but it is usually possible to discover this from trace patches. Little trouble has been caused by wild birds. For example, during a period of snow there were numerous tracks of sparrows, starlings, and robins all round a dozen pipes on a dump. Yet when most of the rats had been eliminated the postbait take was consistently zero except at a single pipe visited by rats. On two occasions, however, postbaits in pipes were heavily raided by sparrows.

(ii) *Waste*

Brown rats, like albinos (Osborne and Mendel, 1919) may not eat the entire wheat kernel. The seed coat and varying amounts of endosperm may be rejected, the proportion of residue varying greatly between populations and with the kind of wheat (Table 38). Rats have two methods of feeding on wheat or baits other than flour or meal: either they stay at or near the supply (settled feeding) or they carry away mouthfuls, generally under cover (bait carrying). Consequently the amount of rejected residues included in the weighing will vary according to the habits of the rats and how the bait is offered. Rats often sit inside a P3, which will then be found to contain a great deal of damaged grain; but at other times these residues are scattered along the runways and under cover and cannot be weighed. Again almost the entire kernel may be consumed, especially with token baits. McDougall (1944) finds that little wheat is wasted by Australian species of rats.

An experiment to determine the amount of wheat rejected was carried out in an outdoor cage containing eighteen rats. Each day fresh wheat, sieved to remove small grains, was placed in a tin tray after the uneaten wheat from the previous day had been set aside. At intervals the damaged residues were sieved out through $\frac{1}{10}$-in. mesh and the proportion of this waste to the total take was found (Table 6). In December and January the rats had a choice of wheat in one tray and flour in the other; but in February wheat was in both trays, one near the nest box, the other at the far end of the cage. The same stock of wheat was used on both occasions but there was an obvious difference in the proportion of waste in the earlier months (8–10%) compared with that in February (15–16%) and there was also a slight difference between the two trays in February.

TABLE 6. *Proportion of damaged grain recovered from uneaten residues*

Date (1944–5)	*21–30 Dec.*	*31 Dec.–11 Jan.*	*20 Feb.*		*21 Feb.*		*22 Feb.*	
Tray	..	..	*Near*	*Far*	*Near*	*Far*	*Near*	*Far*
Total take (g.)	3,570	2,685	295	110	240	150	295	185
Damaged grain (g.)	365	215	40	20	35	25	45	33
Damaged grain (% of take)	10	8	13·5	18·2	14·6	16·7	15·2	17·8
			15		15		16	

Provided the waste is recovered no serious error is involved. The difficulty is that the amount of waste which is dropped outside the containers may differ between individuals and colonies. The extent of this error may reasonably be expected to remain constant for any colony which is baited in the same manner throughout and it is also doubtful whether, under field conditions, very much of the damaged grain is unaccounted for. This is because certain rats have the peculiarity of scavenging whole and broken grains dropped by other rats, rather than, or in addition to, feeding from the communal supply (Appendix 2).

The difference in amount of waste included in the wheat residues may to some extent affect any comparison of abundance between different populations, especially when different types of containers are used, but it is probably one of the smaller variables. It is of more practical importance that the residues, being the less palatable parts, must not constitute too high a proportion of the bait. One of two consequences may follow the repeated return of damaged residues to a census point: either the rats will not eat freely, or if alternative food is scarce, they may take and waste a great deal more bait than usual. The preference for a fresh sample of grain was tested in the field by filling one of a pair of P3s with a high proportion of damaged residues and the other with fresh wheat. The experimental P3 accounted for half the total take for the 4 days before and after, compared with one-quarter on the night of the substitution. Certain low average takes of bait left down for 2 days or more might be explained by the increasing concentration of rejected grain, and it is obviously advisable, especially where there is good natural food, to renew the wheat supply so that it is always attractive.

Baits placed in rat holes are more seriously wasted because they are trampled into the soil and scattered. The amount of waste can be checked as follows. Rats offered a choice of two foods in containers will take less of one the more they eat of the other. With cereal baits, in fact, the total take of solids is more or less constant; but when one of the pair of baits is offered in a container and the other is placed in the burrows the relationship appears to break down. When inspection shows that all the baits have gone from the burrows it will be found that this 'take' added to that from the container is generally very much above the previous level of feeding, the difference, of course, representing the waste. Table 7 gives two of eleven similar instances in the outdoor cages, where a waste of over 50% of the hole baits was common. Two examples studied in the same way in the field again show that hole baits may be wasted (e.g. Table 28).

TABLE 7. *Waste of sugar-meal in earth burrows in outdoor cages*
(*daily takes in g.*)

	Wheat alone	*Wheat and sugar-meal*			
Wheat	180–210 (mean 198)	130	215	160	230
Weight of sugar-meal given	—	120	. .	120	. .
Total take in excess of mean	—	52	17	82	32
Wheat	210 (on each of 3 days)	170	220	120	200
Weight of sugar-meal given	—	150	. .	150	. .
Total take in excess of mean	—	110	10	60	−10

(iii) *Hoarding*

As already stated, rats carry wheat away by the mouthful, and we must now consider whether the amount carried is more than the individual is to consume immediately. First we may examine an instance in which a hoard of grain was recovered. Bait was being laid in a storeroom half full of sacks of potatoes which were turned over periodically. Dry wheat was given first and there were several irregular takes (Table 8 (*a*)) totalling 475 g. of which 230 g. was recovered. This was followed by much lower takes of soaked wheat (5–30 g.), then by further irregular takes of dry wheat. When the sacks were turned over again more dry wheat was recovered (120 out of 335 g. taken) but none which had been soaked. This time a large rat was killed and trace patches proved that the store was now clear of rats. Two similar instances have occurred, both in small populations.

In the outdoor cages we have occasionally recovered wheat from the nest boxes. In such cases the wheat takes on preceding days have been very irregular or have been high at first as in the following series: 1,300, 315, 480 g. In this case nineteen rats had been on short rations for some days (Table 17) and, on the second and third days after a surplus was restored, no doubt fed partly from wheat hoarded on the first day. From the fourth day no further hoarding occurred. In other cases rats have hoarded wheat after a shortage, particularly when poison bait had been offered in the absence of alternative food. At other times the habit has arisen without obvious cause. The postbaits in Table 8 (*b*), after poisonings in the field, suggest that the initially high takes and later irregularities were due to hoarding.

TABLE 8. *Daily takes of census bait (g.) selected to illustrate special effects*

Bait		*Remarks*
	(*a*) *Hoarding*	
Wheat	30, 0, 90, 135, 70, 75-75 (Total 475)	See text
Soaked wheat	20, 30, 15, 10, 5 (,, 80)	
Wheat	73-73-74, 0, 45, 70 (,, 335)	
	(*b*) *Suspected hoarding*	
Wheat	445, 40, 20, 195, 260, 115, 50, 95, 170, 160, 140, 155	Postbait after zinc phosphide
,,	0, 225, 120, 50, 90, 40, 70, 60, 105, 75, 195, 60, 90	Postbait after squill extract
	(*c*) *Abundant natural food*	
,,	145, 55, 15, 0	Flour
,,	160, 50, 0, 15	Meat
1:2 rusk	1,080$^+$, 850, 460, 630, 320	Margarine, dried eggs
	(*d*) *Little natural food*	
Wheat (*a*)	835$^+$, 860, 1,135, 668, 805	Hedgerow round an allotment. Four censuses between 10 Nov. and 7 Jan.
Wheat (*b*)	800, 1,010, 785, 620, 690	
Wheat (*c*)	715, 405, 515, 690-690, 555-555	
Wheat (*d*)	850, 925, 835	

Hyphens show average takes; $^+$ means no surplus.

If hoarding were at all common then census baiting would be useless, but fortunately we can be confident that only a small proportion of tests is invalidated in this way. This conclusion is based on several lines of evidence, perhaps the least convincing being the scarcity of direct proof despite several thousand bait-days. Although it would be largely by chance that such stores were found, more than three might have been expected to turn up. Pisano and Storer (1948) excavated thirty-four burrow systems in poultry yards and found no food stores.

The experimental rat colony affords good proof of the fact that rats usually carry away only what they are to eat at the time. When watching the rapid trips between bait and cover, it was hard to believe that all the wheat was being eaten. At the end of a period of watching such cover was examined, but always without finding anything but rejected residues.

The generally steady level of take is a further indication that hoarding is uncommon, for it seems unlikely that a constant additional amount of wheat would always be laid aside. In the known cases of hoarding the takes show those abnormal fluctuations which might be expected if excess bait was taken one day and consumed later on. There is also the close correspondence, in terms of cereal content, in the amount of any pair of baits offered on different days (Ch. 7). Flour is not an easily portable bait, and though mouthfuls are occasionally finished away from the supply, the rats mostly sit down and lick it up or lift it in their forepaws. When such a bait is given before or after any of the portable ones we would expect to find a great difference in take if the latter were hoarded. In fact, such cases are very rare.

Finally, in a rat population of even moderate density it seems unlikely that any rat would be able to build up a hoard of bait faster than others would remove it. This argument does not, of course, apply to isolated individuals, especially those left after a poisoning, and it is probably significant that the instances of proved or suspected hoarding in the wild concerned small populations.

Various authors have studied hoarding among white rats; the paper most relevant to our own experiences shows that it usually begins after several days of restricted feeding and ceases some time after hunger has been satisfied (Morgan, Stellar, and Johnson, 1943).

(*b*) **Variations in intake of bait**

Although it is not always essential to attract a population entirely away from its natural food supply, the census will fail if the rats do not, after the preliminary phase, take a more or less constant proportion of census bait in their diet. In certain environments this desired equilibrium is not attained without some adjustment of technique.

(i) *Miscellaneous factors*

Even under carefully standardized laboratory conditions white rats show considerable daily fluctuations in intake and the same was true in our outdoor cages. Female albinos have a burst of activity every 4–5 days correlated with oestrus (Wang, 1923; Slonaker, 1924) and intake is lower at this time (Russell, 1948). As discussed later there are other variations in intake connected with reproduction. Kinder (1927) has shown that more food is eaten at lower temperatures but that there is an increased amount of nest building which minimizes the effect. Huddling also raises the temperature of the microclimate as total heat production is the same whether brown rats are in groups or apart (Benedict and Fox, 1934). An inverse correlation between maximum temperatures and food intake was found in our outdoor cages (Ch. 6).

Wang (1925 *b*) found that differences in activity were related to food intake, which according to Hitchcock (1927) increases in proportion to the cube of activity. Activity itself is depressed by light (Browman 1937, 1942; Slonaker 1925 *b*) and high temperature (Kinder, 1927). Rats, on a diet of bread, were more active than those getting meat as well (Stewart, 1898). Hitchcock (1926) showed that the addition of meat had no effect upon activity if the diet already contained adequate protein, but (1928) that protein starvation (also complete starvation) considerably increased activity. Richter, Holt, and Barelare (1938) found no difference in activity between rats on McCollum or self-selected diets, but alcohol depressed activity (Richter, 1926). Slonaker and Card (1923) found a lag of 2 months before a restricted diet depressed activity.

In the field and outdoor cages the daily level of feeding was sometimes upset by environmental changes.

(ii) *Variability of natural food*

Where rats have varying kinds and amounts of alternative food the take of census bait also varies, and in most cases this error cannot be allowed for. The surprising fact is that the majority of takes varied as little as they did. Low takes are clearly not representative of the phase of equilibrium it is wished to estimate, but they are immaterial when peaks are being considered. On the other hand, a peak take may equally well depart from the general equilibrium because of a sudden shortage of natural food. The census bait must therefore be sufficiently attractive to form a high proportion of the rats' total intake.

(iii) *Abundance of natural food*

Rats living in 'general' urban and rural habitats are less difficult to prebait than are those living among unlimited food supplies. By 1941 large stocks of flour, sugar, and other sacked and tinned goods had been collected in 'buffer depots' throughout the country as a precaution against the effects of bombing or invasion, and Freeman undertook a study of certain infestations among them (e.g. Table 45). Buffer depots are a heterogeneous collection of buildings ranging from those originally designed for food storage (cf. Plate 8*a*) to empty shops, cinemas, or church halls. They may vary in capacity from 10 to 10,000 tons, and their contents may remain in the same piles for considerable periods. According to where they lived, rats were described by Freeman as 'inside' or 'outside'. 'Inside' rats lived entirely within a food supply consisting of one or a few types of grain or flour, or at least were entirely dependent on it for food. 'Outside' rats might feed mostly on grain or flour, but lived outside their supply in an environment where mixed foods were available.

In the course of his work he encountered the peculiarity of a fall off in the take of certain baits by 'inside' rats (Table 8 (*c*)). The take on the first day was usually the highest and then dropped, sometimes to zero. The same thing occurred later in the Oxford market (§ 16) and, among other places, in a slaughter house, a grocery, and a store of margarine and dried eggs. In an experiment in the outdoor cages some rats which had been kept on bread, flour, and cabbage took more wheat on the first 2 days after it was added to the diet than they did in the week following. On two later occasions the wheat supply was interrupted for 2 or 3 days and again there were high initial takes followed by a fall. These high wheat takes caused a corresponding drop in the food taken from the other sources.

There is at present no explanation for the rats' initial interest in a food which they may refuse later. It seems unlikely that so short an interest would correct dietetic deficiencies, though these presumably occur in buffer depots containing white flour only. In two outdoor cage tests nutritional deficiencies may have been responsible for a high take of cabbage after a long period on wheat alone (Table 9); but in a third experiment the take of cabbage did not fall off in the same way.

TABLE 9. *Takes of cabbage in two outdoor cages after several weeks on a cereal diet*

No. of rats	*Mean body wt. (g.)*	*Mean take of cereal (g.) in previous week*	*Takes of cabbage (g.) Initial daily takes*	*Range for next week*
8	225	155	365, 320, 260, 200	180–270
9	400	256	520, 410, 370, 280	220–325

Scott and Verney (1947) claim that for a night or two their white rats relished a change in the carbohydrate portion of their diet and that choice was independent of nutritional factors. Young (1946) has made an elaborate study of the effect of habit on food choice.

Freeman found that with baits of soaked wheat an equilibrium was established similar to that with dry wheat in other habitats. In the buffer depots the rats were mostly accustomed to a single type of food, but the same need for soaked wheat as a census bait was found in some places where a variety of foods was attacked. The reasons for the superiority of soaked wheat as a bait have not been investigated. The water content is not the attractive feature since other wet baits may not be taken at all well. It is also unlikely that the texture is the deciding factor (Carlson and Hoelzel (1949) believe this is true of soaked as compared with dry maize), nor is there generally any increase in nutritive value brought about by soaking (Maynard, 1937). Most probably there is some difference in taste due to changes from starch to sugar.

(iv) *Scarcity of natural food*

High takes followed by low takes have also occurred where food was not obviously abundant, or was definitely scarce, and it was later proved that hungry rats may eat abnormal amounts for the first few days of baiting. The type of result which led to an investigation of the effect of hunger is shown in Table 8 (*d*) and experimental proof is given in § 7. Such takes do not show the continual falling off found in the buffer depots, nor are they or the subsequent figures as irregular as might be expected if the colonies were hoarding bait.

Peaks which are suspected of being caused by hunger may occur immediately after a period of token baiting. The token baits attract rats to the baiting points without adding much to their diet so that nearly all take a large meal on the first night of surplus baiting (§ 4 (*a*)).

(*c*) Summary

In this section we have tried to find out if peak takes of census bait are proportional to population density and have found some evidence that this is not always so. Total bait intake may vary slightly because of differences in individual requirements and to a greater extent when the whole population is affected at once by environmental changes such as in temperature or natural food supply. When a bait is given for the first time or after an interruption some rat populations may take larger amounts at first than they do later. Where certain types of food are abundant, rats may take the first baits only and then return to their accustomed diet. Where food is scarce rats may have abnormal appetites or may hoard the baits (though the latter habit may occur where food is plentiful). The field evidence is sometimes clear that the rats are hungry; sometimes they are obviously well fed; elsewhere it is impossible to assess their food supply. Unexplained irregularities in take may be caused by a number of factors such as disturbance. Nevertheless it is usually possible to overcome these difficulties (§ 8).

7. FOOD AND WATER REQUIREMENTS

(*a*) Introduction

By adopting a form of census based upon the feeding behaviour of the rat we had to make some study of its diet and nutrition. In this large, complex, and rapidly changing field none of us had had much experience and the following survey is therefore incomplete and rather unselective. An immense amount of work on the white rat provides a unique background for field studies on the wild race; but as there appeared to be a number of gaps in the type of information most relevant to our own studies it became necessary for us to carry out certain experiments on captive animals. By the standards of physiological research these experiments were naturally crude. The outdoor cages were subject to most of the variations of an uncontrolled environment, the rat stocks were obtained from different habitats, consisted of different age and sex groupings, and probably suffered from lack of proper diet in proportion to the length of their captivity. Moreover there were errors in measuring food and water takes; and, because of the difficulty of obtaining individual data, body weights were frequently averaged from a fairly wide range. Some disturbance was undoubtedly caused by weighing the rats and there were other variable factors such as pregnancy, lactation, differential growth rates, cannibalism, and numbers per cage.

However, our chief aim was to duplicate effects which had been observed in the field despite numerous other uncontrollable factors. Also we were less concerned with establishing new principles than with seeing whether observed effects conformed roughly with expectation from the precise studies of the physiologists.

An animal's food must at least provide enough salts, vitamins, proteins, and energy; but an upper limit to the intake of any of the first three is set by the amount of energy which the body can expend. Except for short periods it is impossible for an animal to eat abnormal amounts of its diet merely to obtain essential items which occur in too small a proportion (Osborn and Mendel, 1915 *a*; Boas-Fixsen, 1935). Food in excess of the normal bodily requirements is diverted to fat formation unless used up through additional exercise.

In the present section the term growth means increase in body weight. However, Orr and Richards (1934) have pointed out that skeletal growth may continue after weight stops increasing or is actually lost.

(*b*) **Growth and reproduction on a wheat diet**

(i) *Nutritive value of wheat*

Because wild rat populations in ricks seemed to live on a pure wheat diet we did not at first question the adequacy of wheat as their sole food in captivity. Wild-caught animals lived in apparently good health and vigour for the few weeks which, up to 1944, was the time any were kept before being killed. However it is clear from the literature that wheat is deficient at least in salts and vitamin A and therefore that some source, not always evident, must be available to corn-rick populations. The high reproductive performance of these rats (Venables and Leslie, 1942) is of interest in comparison with the relatively poor rate of breeding, weaning, growth, and survival of white rats on purely vegetarian diets (Carlson and Hoelzel, 1947, 1948; Schweigert, 1947).

Hart and McCollum (1914) found that on wheat and distilled water white rats made no growth and rarely survived beyond 60 days. Hart and Steenbock (1919) found that rats on this same diet maintained their weight for not more than 4–8 weeks and died within 28 weeks. They give many references to the failure of fowls and mammals to make normal growth on a diet of grain only. When the rats' diet was corrected for salt and vitamin A deficiency, growth was obtained on the proteins supplied by a diet with 90 or 92% whole wheat (McCollum, Simmonds, and Pitz, 1916; Osborne and Mendel 1919, 1920) though mothers failed to rear their young on this diet. This was believed to be due to some limiting factor other than the adequacy of wheat proteins. In a later review of the biological values of proteins it is shown that those of wheat are of a very high order (Boas-Fixsen, Hutchinson, and Jackson, 1934).

(ii) *Added salts and vitamin A*

In our studies of caged rats we were mainly interested in their daily wheat consumption and were thus unable to give them the complete diets normally fed to laboratory stocks. However, by adding cabbage to the diet the main deficiencies were probably overcome without much reducing the wheat intake. Green and Mellanby (1928) found that 0·5 g. dried cabbage per day contained sufficient vitamin A for a white rat. This represents a maximum of 6·25 g. fresh cabbage. Another estimate may be obtained from McCoy (1942) who gives 20 μg. carotene per kilo body weight as the requirements of white rats. This amount of β-carotene is contained in approximately 37 g. cabbage (Medical Research Council, 1945) so that rats of 200–400 g. body weight require 7–15 g. cabbage per day.

(iii) *Effect of proteins and energy*

Though deficient in salts and vitamins, wheat is a superior source of proteins and energy. While the deficiencies create one problem with caged rats, the good qualities create another when wheat is fed to wild animals with a limited natural supply of proteins or calories.

Where growth has been retarded through lack of essential food elements it may be resumed at an abnormally rapid rate after the deficiencies have been corrected. Rats kept at constant body weight on a protein-free diet grow faster on full refeeding than controls of the same weight (Jackson, 1936) and similar acceleration in growth rate occurs after prolonged restriction of calories or salts (Osborne and Mendel, 1915 *b*, 1916; Clarke and Smith, 1938; Jackson, 1937, 1939). Medawar (1942) points out that this is part of a general process which he terms 'post inhibition growth rebound'.

When wheat supplies an item missing from the natural diet a rat population may be expected to increase its growth rate, metabolic rate, and food intake. Evidence is given in § 15 of rapid weight increase in wild rats which had been partly starved; and similar results may have been produced elsewhere through correcting a lack of high quality protein.

(iv) *Growth rates of adults*

Rats in our outdoor cages were not as strictly limited for salts as were those of Hart and his co-workers and evidently their stored vitamin A was sufficient for several weeks' growth. Brenner, Brookes, and Roberts (1942) have shown that although some young rats may be made to lose their liver reserves of vitamin A in one week, sufficient amounts of it are maintained in the eye and blood to allow weight increases for 7 weeks. Animals that had been fed massive doses of vitamin A had adequate liver stores even after 30 weeks of vitamin A free diet. Table 10 gives the body weights (g.) of three animals at capture and after 5 and 10 weeks on a diet of wheat and tap water and such items as their litter, excreta, and cage structure may have provided.

TABLE 10. *Growth on a wheat diet*

Day	*0*	*35*	*70*	
♀	120	145	185	neither pregnant
♀	165	200	235	
♂	145	195	250	

Two litters of cage-reared rats also continued to make fairly good growth on a cereal diet (Tables 11 and 12). Growth rates, however, were rather less good than those of the twenty-fifth generation captive brown rats quoted by King (1939). Moreover, as pointed out by Leslie, Perry, and Watson (1945), to judge by the size of King's largest rats they belonged to a smaller strain than did some of our local populations.

TABLE 11. *Body weights, food and water takes of eight young rats (3 ♂♂, 5 ♀♀. Outdoor cage 1)*

Mean temp. (° F.)		*Mid-class age (days)*	*Mean body wt. (g.)**	*Mean daily wheat take (g.)*	*Mean daily water take (c.c.)*	*Mean daily cabbage take (g.)*
Min.	*Max.*					
51	63	33·5	53	75·5± 8·1	62·0±4·3	..
38	52	43·5	72	113·0± 8·5	72·0±6·5	..
51	62	53·5	88	117·5± 3·8	105·5±5·6	..
52	64	63·5	107	125·5±10·0	127·5±6·5	..
49	60	73·5	134	145·5± 4·7	73·0±5·3	179·0± 9·2
54	62	83·5	154	160·0± 4·9	104·0±7·0	153·0± 7·3
51	67	91·5	169	152·5± 8·3	113·3±7·9	150·0±16·6

* By interpolation from part of the following data:

Age (days):	0	7	10	15	21	28	42	49	56	63	70	77	84	99
Mean body wt.:	?5	12·5	15·9	22·5	28·0	42·5	69·2	80	93	106	125	144	156	184

Food requirements changed less rapidly for a given weight increase in the heavier animals in our outdoor cages (Ch. 6). In the laboratory no difference in take was found between the sexes; but in albinos Wang (1925 *a*) and Richter (1926) found that females required more per kg. body weight. Richter quotes 174 cals./kg. for males, 215 cals./kg. for females.

(v) *Reproduction*

For successful reproduction it is probable that a cereal diet alone is inadequate, though two litters survived despite the fact that the females had been given no vitamin A for 3 months and none until part way through lactation. Dann (1934) found that the rearing of young does not cause a serious drain on the vitamin A reserves of the mother rat. In our cages only two other litters were successfully reared; these were born to females which were receiving cabbage or cod-liver oil. The mortality amongst all the many other litters may well have been because dietary deficiencies lowered their viability (Russell, 1948) or caused the other rats to eat them. On the other hand, King (1939) found that when she removed the nestlings of the early generations the mothers ate them on their return. Two of the litters born to our wild-caught females were handled from birth without this happening, but some of the other mortality may have been due to human interference.

TABLE 12. *Food and water takes by eight rats (1 ♂, 7 ♀♀. Outdoor cage 4)*

Date (1945)	*Age** *(days)*	*Mean body wt.** *(g.)*	*Mean daily cereal take* *(g.)*	*Mean daily water take* *(c.c.)*	*Cabbage*
10–16 Mar.	57	104	124±3·3	148± 3·4	None until 26 May: then 259 g. average per day until 1 June; average water take for same period 105 c.c. (169 c.c. in previous week).
17–23 ,,	64	..	..	..	
24–30 ,,	71	..†	72±8·0	71± 2·8	
31 Mar.–6 Apr.	78	..†	84±5·2	72± 4·1	
7–13 Apr.	85	..†	83±4·2	76± 5·8	
14–20 ,,	92	..	143±6·3	171± 5·6	
31–27 ,,	99	..	158±7·1	169± 7·5	
28 Apr.–4 May	106	200	167±8·6§	146±15·1	
5–11 May	113	206‡	148±6·1	174±10·0	

* On last day of period.

† Four rats only, mean body wt. 119 g. (20 Mar.); 171 g. (13 Apr.). Mean for all eight 159 g. (13 Apr.). See Table 15.

‡ First of seven litters born 8 May.

§ Sugar-meal only.

Overcrowding may have been responsible for further deaths, though three of the four successful litters were born in cages containing eight adults. One other litter was born in a cage which contained eighteen adults at first; on the day of parturition the female and young occupied the usual end of the nest box and the other seventeen rats the end which was not normally used.

We have little information ourselves on the changes in food requirements due to reproduction, but the literature shows that pregnancy makes a much smaller increase in the daily intake than does lactation. Richter and Barelare (1938) found that on a self-selection diet the average daily intake rose from a normal 45 cals. to 60 cals. during the last 5 days of pregnancy and to 118 cals. during the 5 days before weaning on the 25th day. There was also a change in the constituents of the diet (Barelare and Richter, 1938). Wang (1925 *b*) found only a slight increase in food intake during pregnancy (1–13·5%) and an actual drop in terms of cals./kg. body weight which he attributed to the energy conserved by a 90% reduction in activity. During lactation intake rose up to 134%. Slonaker (1925 *a*) similarly demonstrated a reduction in activity and only a slight rise in food intake from a normal 18 g. per day to 20 g. during pregnancy. With litters over six, the females ate an average of 34·6 g. from the first to twentieth day; those with less than six young averaged 25·3 g. per day. Slonaker (1929) also describes a 25% increase in intake and a gain of 2 g. body weight per day during the first week of pseudopregnancy, a condition which results, for example, from mating too early or too late in oestrus (Cooley and Slonaker, 1925).

Russell (1948) has redrawn the data from Slonaker (1925 *a*) and reviewed other literature. The intake of the rat is shown to be as high as 65 g. per day towards the end of 4 weeks' lactation compared with 12–20 g. during gestation. It is not made clear, however, that after the twentieth day Slonaker's figures include some direct feeding by the young on the stock diet.

Cole and Hart (1938) found that on the second day of pregnancy there was an increase in food intake which they attributed to the effect upon appetite of a hormone released as a result of coitus. By the eighteenth day of pregnancy the rats were eating 50% more than their littermate controls. Intake fell on the day of parturition, then rose to 3 times that of the controls on the twelfth day and remained at about this level until weaning.

When the young are weaned the food intake of the parent drops while that of the young increases. With white rats Wang (1925 *a*) has shown that food requirements rise quickly between 20 and 50 days and then remain fairly steady up to 220 days. A similarly rapid increase in food requirements was found in our outdoor cages (Table 11).

(*c*) **Normal relationships between takes of food and water**

(i) *White rats*

Richter (1926) measured the water requirements of white rats and from his graph it appears that 30–100 c.c. per day were taken by animals with a food intake of 10–16 g. McCollum diet. Between 30 and 160 days of age, body weight increased faster than water intake but there was a correlation with body surface of 800 c.c. per sq. m. per day (Richter and Brailey, 1929). Richter (1938) gives further data which place the figure for rats, man, and certain other species at 1,050–1,238 c.c. per sq. m.; e.g. rats averaging 225 g. drank 35·3 c.c. McCay and Eaton (1947) quote figures of 12–19 c.c. per 100 g. body weight according to age. Fats perhaps lower the water requirements (Richter and Barelare, 1938) and salts certainly raise them (Gamble, Putnam, and McKann, 1929).

(ii) *Brown rats*

Ten recently captured rats were kept in small laboratory cages and their water take on a wheat diet was observed for 14 days (Table 13). There were some considerable daily and individual differences which prevent an accurate correlation with body weight or sex. However, the larger rats tended to drink more, and for this limited range of body weights a straight line regression has been calculated for c.c. drunk per g. body weight. The slope $b = 0{\cdot}08 \pm 0{\cdot}025$ is significant for 8 degrees of freedom without weighting the observations ($t_{[8]} = 3{\cdot}2$; $P < 0{\cdot}02$). These rats had a mean body weight of 240 g., a mean water take of 24·5 c.c., and from results with other brown rats in the laboratory (Ch. 6) their mean expected wheat take was 20 g.

TABLE 13. *Mean daily water takes of ten brown rats fed on whole wheat in the laboratory for 14 days*

Sex	*Body wt. (g.)* (x)	*Mean daily water take (c.c.)* (y)	
♂	155	17·43±0·7740	
♂	165	17·78±0·8053	
♀	182	21·36±0·9637	
♂	192	19·93±0·6992	$\bar{x} = 239{\cdot}3$
♀	248	26·43±2·0015	$\bar{y} = 24{\cdot}54$
♀	265	35·86±2·2776	If $y = a+bx$
♀	272	22·00±2·1497	then $b = 0{\cdot}07986 \pm 0{\cdot}02510$
♀	272	21·86±1·2404	
♂	320	35·00±2·1071	
♂	322	27·71±1·0869	

Mean expected wheat take (Ch. 6): 15·5 g. at body wt. 155 g.; 23·8 g. at body wt. 322 g.

Takes of wheat and water were measured simultaneously in another test and found to be respectively 26 g. and 37 c.c. per rat per day (Table 14). In the laboratory, therefore, adult brown rats seem to need between 1 and 1·5 c.c. water per g. wheat. Food requirements are known to be less in the laboratory than in the outdoor cages; but it is probable that more water may be needed at the higher indoor temperatures.

TABLE 14. *Takes of whole wheat and water by two rats (♂♂ 210, 310 g.) caged together in the laboratory*

	Mean daily take (12 days)		*Mean daily take (11 days)*	
	Wheat (g.)	*Water (c.c.)*	*Soaked wheat (g.)*	*Water (c.c.)*
Observed . .	52·6±1·5	73·8±2·1	67·2±1·9	51·7±4·9
Corrected to dry cereal content* .	45·8	80·6	41·8	77·1

* Assuming 13% natural moisture (Medical Research Council, 1945) and 40% increase on soaking (Ch. 7).

Water requirements in the outdoor cages are, indeed, often proportionately less but the data cannot be considered adequate for a comparison. Tables, 11, 12, 18, and 19 show variations above and below a take of 1 c.c.

water per g. wheat, but under normal conditions the averages do not depart very widely from this relationship, which is sufficiently good as a rough guide. Much the same figure is found with sugar-meal (Tables 12, 19).

Moist cereals. Baits with a higher water content than wheat may reduce the amount of water taken from the drinking vessel. Table 14 shows that when soaked instead of 'dry' wheat is given the drop in water take is accounted for by the difference in water content of the two baits. In this example the natural moisture content has been taken into account as well; but for the purposes of this discussion it is generally sufficient to correct only for added water.

An equal weight of water mixed with rusk might be thought to be enough by itself; but for preference rats still drink a certain amount as well. Despite an exclusive diet one day of 535 g. (1 : 1) rusk, some caged rats also drank 190 c.c. water. This seems to have been unusually much, but when wheat and rusk are given over a period of days, the combined amount of water taken in the rusk and from the vessel is always slightly and perhaps significantly above 1 c.c. per g. cereal. This fact is brought out in the case of four rats (Table 15) separated from their littermate controls (Table 12) and given wheat, water, and rusk.

TABLE 15. *Water taken with a moist bait*

		1 : 1 Rusk		
	Wheat	*Cereal*	*Water*	*Water*
31 Mar.–6 Apr.	56	43	43	64
Totals	99±7·9		107±4·1	
7–13 Apr.	41	50	50	54
Totals	91±9·9		104±9·7	

The combined take of rusk and wheat is larger than might be expected from a slightly lower body weight of these experimental animals, suggesting some error in the take of rusk. However, per g. wheat alone more than 1 c.c. water was taken whereas the controls (Table 12) took 83–84 g. wheat and 72–76 c.c. water for the same periods.

Cabbage. Cabbage was given on several occasions in addition to wheat, but its water content, though 92%, does not displace an equal amount of water from the diet. Rats in the refeeding experiment in Table 17 took, or were given, variable amounts of cabbage, and for the 3 weeks of this period during which water takes were also measured, there was a rough inverse relationship between the two. The slope of the straight line regression showing the decrease in c.c. water per g. cabbage was found to be $b_2 = -0{\cdot}38 \pm 0{\cdot}06$ ($t_{[19]} = 6$; $P < 0{\cdot}01$).

If this relationship held throughout, 650 g. cabbage would displace the normal water take of 245 c.c., though it is unlikely that rats would voluntarily confine themselves to cabbage—at any rate for long, as salts in the cabbage itself probably demand additional drinking water. On the only occasion when rats took cabbage but no water, they drank 500 c.c. or twice the usual amount next day. (This happened when a surplus of cabbage was given instead of a restricted amount.)

Various other data show how cabbage displaces from the daily take only a part of its own weight of water. The rough relationship can be seen from the mean difference in water take with and without cabbage in the diet. Thus in three different groups of rats an average daily take of 361 g. cabbage displaced 80 c.c. (Table 19) water compared with the previous week, 259 g. displaced 64 c.c. water (Table 12), and 179 g. displaced 54·5 c.c. (Table 11). These differences and that in Table 18 are rather less than were found before but are of the same order (0·2–0·3 c.c. per g.) and about as close as can be expected from this rough method of comparison.

(*d*) **Effects of restricted food or water**

(i) *Literature*

If food is restricted water consumption falls off. Rats losing their appetite through vitamin B deficiency and control rats restricted to the same amount of a complete diet, both show falling water intakes; and in the recovery phase food and water intakes show a parallel rise (Rose, Stucky, and Mendel, 1930). Kleitman (1927) has shown that the starving dog drinks very little and Benedict and Ritzman (1923) that steers drink much less when underfed. Jackson and Smith (1931) found that restriction of either food or water reduced the intake of the other, but that with prolonged restriction of water rats were able to maintain their weight on progressively smaller quantities. They obtained lower figures for normal water intake than Richter (1926) or Richter and Brailey (1929).

Bing and Mendel (1931) found that rats on a ration with 9·6% water content normally required 10–14 c.c. water per day and ceased eating after the third day of water starvation. With different restricted amounts of water there was a quantitative relationship between intakes of food and water. With the mouse they have shown that the intake of water is much higher than the amount necessary for excretion of CO_2 and urea and conclude that the excess is needed to moisten the food so that it can be ingested.

Few data are available on the daily food intake of animals recovering from underfeeding. It is to be expected that, while gaining or regaining weight, more food per unit body weight is needed than for maintenance alone. Famiani (1932) found that pigeons, after a 20% loss in weight due to fasting, eventually took double their normal rations until, or even after, normal body weight was regained. However, rats which were growing unusually rapidly after 15 weeks of restricted food (Jackson 1937, 1939) seem to have taken no more than controls growing at the normal rate.

Adolph (1947) found that after rats had been starved for 24 hours th 'food deficit was never paid off by subsequent overconsumption' and tha weight was regained only slowly through using food more economically When only half the ration of water was given for 23 days, food intake fel by a quarter, body weight was lost in the first week but was then slowl regained. When normal water supplies were restored, food and wate intakes were unusually high for the first 5 days. In the complete absence o water, intake of food fell to one-tenth after the third day.

In man 7,500–8,000 cals. per day may be taken during recovery fro severe starvation (Murray, 1947).

Benedict and Fox (1934) found that young white rats were able to li for 17–24 days in the absence of food and had lost 39% body weight death. Brown rats mostly died on or before the sixth day of fasting for, i common with many other wild animals, they have a higher basal metabol rate than captive stock (Benedict, 1938). Body weights at death were abo two-thirds normal.

As it seemed likely that some of our wild rat populations might ha been affected by shortage of food or water it became necessary to see wh errors might be introduced into the census methods.

(ii) *Food taken during refeeding*

When rats in our outdoor cages were deprived of wheat for 24 hours th ate large amounts on the day or days following (Table 16). As a result

TABLE 16. *Effect of 24-hour starvation on daily takes of wheat and wat*
(caged rats)

(a) Wheat (g.) .	220	0	350	200	270	230	240	360
(a) Water (c.c.) .	210	200	340	180	300	240	240	125
(b) Wheat (g.) .	240	0	535	350	100	200	270	310
(b) Water (c.c.) .	280	170	330	300	150	270	200	170
(c) Wheat (g.) .	175	212	0	253	100	205 - - -	205	205

these large meals the food deficit was soon made up and the average ta for the few days after and including the day of starvation was little, if all, different from normal. Two longer-term experiments were undertak in order to see the effects of chronic food shortage such as might occur nature. In a preliminary experiment nineteen rats were given only slight less than normal for several days. No details of body weight and food r quirements are available until the day refeeding began and only the avera take is known until after the third day when hoarded grain was found a corrected for. The animals were weighed every 3 days at first and th every 6 days until the twenty-seventh day. Water was always present b variable amounts of cabbage were supplied (Table 17).

There was a considerable gain in body weight during the first 3 day much of it, without doubt, due to increase in gut content. Takes were high for 6 days, fell progressively below that expected for rats of this body weight and finally reached a fairly stable level after the twenty-fourth day. This level, however, was far below that normally found and a correction for the calorific value of cabbage makes no appreciable difference.

TABLE 17. *Regrowth and wheat take after a restricted diet*

(*Nineteen rats in cage 3, 23 November–27 December 1944*)

Day	*Mean body wt.**	*Mean daily wheat take*	*Wheat take per rat*				*Mean daily cabbage take per rat*	*g. wheat of equal calorific value*
				Expected				
			Observed	*Mean*	*Range*	*Observed–expected*		
1–6	267	623	33	28	25–31	+5	7·7	0·61
–12	288	440	23	29	26–33	–6	17·4	1·37
–18	287	337	18	29	26–33	–11	20·5	1·61
–24	302	380	20	30	27–33·5	–10	15·6	1·23
–30	312	451	24	31	27–34	–7	9·3	0·73
–36	..	410	22	..	..	..	..	..

* On third day of each period (i.e. days 3, 9, 15 . . .). Body weight on day 0: 238 g.; on day 6: 280 g.

This comparison of takes with those found previously in other rats is not satisfactory, as the effects of specific conditions cannot be allowed for. Some months later the experiment was therefore repeated in better detail. Two of the rats had died; the remaining 17 were divided at random into 2 groups: 8 controls and 9 experimentals. Both groups received 100 g. cabbage per day; the controls always left part of theirs, though capable of eating far more than this. After a period of adjustment, the nine rats were given 150 g. wheat per day instead of their usual requirement of about 240 g. (Fig. 7). After 15 days they were refed and in 24 hours took about

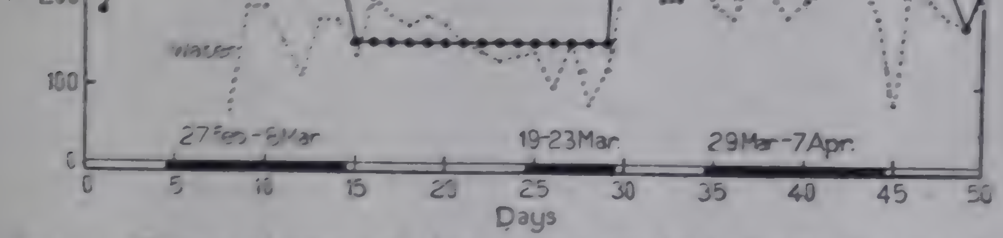

FIG. 7. Effects of restricted food supply on body weight and wheat and water consumption of nine adult rats. Alternate black and white intervals show days for which mean values are given for the controls in Table 18 (where figures are pooled for 19–28 Mar.).

twice the previous daily amount, body weight increasing an average of 26 g., largely because of the increased gut content. Takes remained higher than normal for the first 2 weeks. In the third week feeding was probably back to its original level, but by mistake a surplus of cabbage was supplied from 8 April, and this seems to have caused big fluctuations in wheat take.

The controls maintained an approximately constant body weight throughout, except for one pregnant female. The litter she raised prevents a period to period comparison of food take between the groups; but after the removal of her young it is evident that the adults had not significantly changed their feeding rate since the start (Table 18). It is probable, therefore, that but for the effect of the cabbage, the re-fed animals would have returned to a stable rate of feeding by the third week. The extra supply of cabbage also had marked effects on the wheat take of the controls.

TABLE 18. *Control observations on eight adult rats in cage 3 during food restriction in cage 2*

Date (1945)	*No. days*	*Mean body wt. adults** *(g.)*	*Mean daily cereal take* *(g.)*	*Mean daily water take* *(c.c.)*	*Mean daily cabbage take* *(g.)*
23–26 Feb.	4	..	..	..	..
27 Feb.–8 Mar.	10	408	224 ± 9·3	..	Some
9–18 Mar.	10	418†	228 ± 4·9	178 ± 9·1	62
19–28 ,,	10	400	238 ± 4·2	250 ± 6·0	46
29 Mar.–7 Apr.	10	410	252 ± 6·8	246 ± 6·2	70
8–13 Apr.	6	406	286 ± 18·2	231 ± 65·4	152‡
14–17 ,,	4	..	225 ± 16·4	255 ± 17·1	0
18–27 ,,	10	400	212 ± 6·9	268 ± 10·5	0
26 Apr.–15 June	(see Fig. 8)				

* Weighed on same days as experimentals (Fig. 7).
† Eight young born; removed to cage 1 on 14 Apr. (Table 11).
‡ Surplus instead of 100 g. per day.

These experiments showed how previous underfeeding was followed by a daily take up to twice the normal and by a slow return to the previous rates. It had been suspected that many wild colonies of rats were severely limited in their food supply; but a study of this problem was outside the scope of our work. Since even rats in a corn-rick respond well to prebaiting, it is difficult to prove, from such behaviour, that wild rats are underfed, and our only solid evidence is from the experimental colony (p. 168.) In February 1944 the rats had been without surplus baits for about 2 months, and because of a reduction in food for the livestock there must have been little to eat, though evidently enough to prevent large-scale emigration to the adjacent rubbish dumps.

A marking and watching scheme was then carried out (§ 15) and as late as the fifth night of surplus baiting there was a large take of 51 g. per rat per night instead of the expected upper limit of 34 g. for rats of their weight (Table 32). This was followed by a drop to a lower level of take. In addition certain marked animals were found to have increased in weight beyond what might have been expected from normal growth in 3–4 weeks (Appendix 2). This course of events is typical of rats refed after restriction of food, and although conditions were produced artificially it seems likely that they must be paralleled in many environments where there is a poor food supply.

(iii) *Water taken during and after food restriction*

Less water was drunk when no food was given for 24 hours, but when exceptionally large meals were eaten, water take did not always increase in the same proportion (Table 16). Fig. 7 shows that in the 15 days after food was reduced from 240 g. to 150 g. per day, water take, after an initial drop, rose again and fell off rather gradually. (A constant 100 g. cabbage was given during this time.)

(iv) *Food taken during water restriction*

When deprived of water for 24 hours rats in the outdoor cages took appreciably less wheat than usual. In two groups of rats the following takes were observed (italic type) compared with the days before and after: 195, *135*, 185; 230, *110*, 260 g. wheat. Prolonged restriction of water to about 60% normal resulted in a slight drop in wheat take during the first 2 weeks (Fig. 8). During the third week the rats apparently became adapted to the low water take as wheat consumption rose at times to about its previous level. After a further cut in the fourth week to about 40% normal the rats again ate less wheat; but more than they had in the first and second weeks when they were getting more water. Body weights fell steadily throughout. Table 19 gives data for the controls (the experimentals of the former test).

TABLE 19. *Control observations on nine adult rats in cage 2 during water restriction in cage 3*

Date (1945)	*No. days*	*Mean body wt.,* (g.)*	*Mean daily cereal take (g.)*	*Mean daily water take (c.c.)*	*Mean daily cabbage take (g.)*
23 Feb.–13 Apr. .	(see Fig. 7)				
14–17 Apr. . .	4	..	205†	255	0
18–27 „ . .	10	400	270 ± 16·6	232 ± 17·2	0
28 Apr.–4 May . .	7	383	278†	227	0
5–11 May . .	7	394	286 ± 11·1	251 ± 9·9	0
12–18 „ . .	7	389	229 ± 9·8‡	232 ± 7·4	0
19–25 „ . .	7	389	256 ± 21·8	251 ± 11·0	0
26 May–1 June . .	7	400	268 ± 11·7	171 ± 10·5	361§
2–8 June . .	7	405	261 ± 6·9	226	160

* On last day of each period.

† No wheat on 14 or 28 Apr. (included in means).

‡ Sugar-meal instead of wheat.

§ Surplus cabbage 26 May to 5 June (Table 9).

This experiment was concluded by offering the rats 1:1 rusk, though without restoring their drinking water to normal. Total cereal take rose immediately to a moderately high level and the rats regained body weight. In the following week a surplus of drinking water was restored and the food obtained from the wet bait fell from 1·4 g. to 1·1 g. per g. wheat. Total water take was not unusually high following the prolonged restriction.

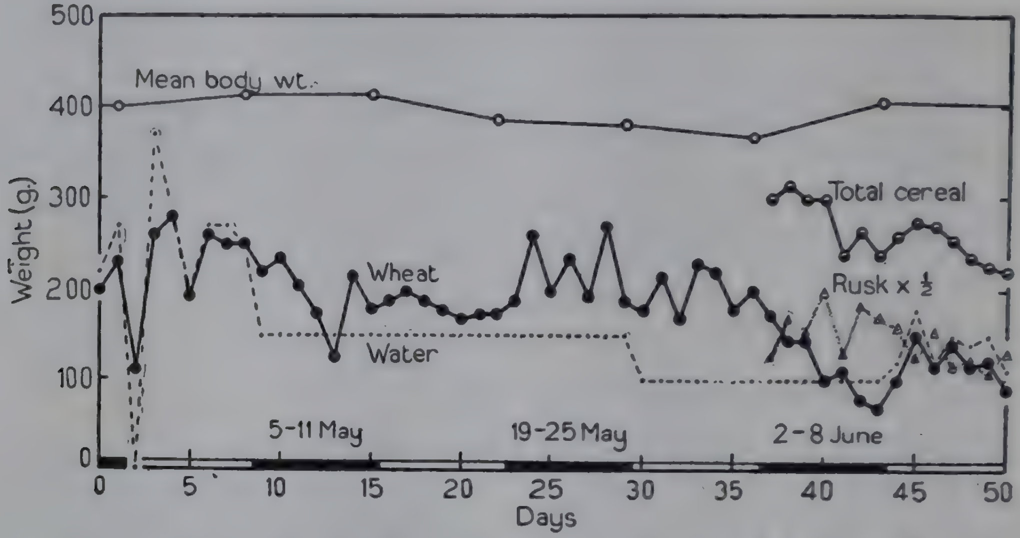

FIG. 8. Effects of restricted water on body weight of eight adult rats and on their takes of wheat and choice of bait. Alternate black and white intervals show days for which mean values are given for the controls in Table 19. Wet takes of 1:1 rusk are plotted × ½, i.e. in terms of cereal content.

Our only evidence about the effect of restricted water supply on wild rats' take of food is again that obtained at the experimental colony. Following a clearance in February 1944 (§ 15) the population slowly built up on the wheat surplus. There was fine dry weather at the end of April and in early May, and no obvious supply of drinking water close to the farm. A choice of 1:1 rusk was given with the wheat, and after the first day resulted in a rise in total cereal take well above previous levels (mean 434 g. compared with 228 g. wheat alone). Later on the take of wheat remained at about 400 g.; but, assuming continued immigration, this is not as significant as the increase to 626 g. in the cereal content of the diet when bread mash was added. Finally, with wheat only, there was a reduced take (500 g. average), then a spectacular jump from 565 g. one day to 870 g. the next when a pan of water was provided. A break in the weather on 26 June made it impossible to repeat this experiment. In view of similar events among our caged rats it may be assumed that lack of water was limiting the amount of wheat this wild colony was able to ingest.

(*e*) **Summary**

1. Methods of measuring the abundance of a natural population must be examined for their possible interference with existing conditions. From

the literature it seemed likely that census baiting might sometimes alter the existing feeding rates and body weights; alternatively that intake might be limited by the amount of available water.

2. The growth rate, reproduction, and survival of adult rats in outdoor cages were sufficiently normal for the purposes of this study, though conditions were not always suitable for the survival of nestlings. About 1 c.c. water was drunk per g. wheat eaten.

3. Restriction of food for 1 or more days was followed by a considerable rise in the daily take and then a fall similar to that found in certain wild colonies. Restricted water supplies reduced the wheat intake, and even with moist baits or cabbage additional water was required. Considerable gains in body weight were made when adequate food or water was restored.

4. These studies suggested that where the natural food or water supply was inadequate, wrong inferences might be drawn from the results of census baiting unless certain safeguards were taken.

8. DESIGN AND VALIDITY OF TESTS WITH CENSUS BAITING

In measuring the effect of a poisoning certain factors affect the design of the experiment as a whole; others more particularly affect the postbait because of the altered condition of the population. Some rats have been removed, the survivors feel ill, and their attitude towards bait may be different; the wheat supply has been renewed after an interruption and the rats have again to adjust themselves to it. These and other factors may affect the new feeding equilibrium.

(*a*) **Effects of changed diet**

(i) *Miscellaneous effects*

Census baiting alters the rats' feeding habits and, especially where food is normally scarce, may affect body weights, times of activity, movements, and perhaps susceptibility to poison. If the population is to be poisoned under conditions typical of the habitat it is obviously necessary to stop census baiting before the poisoning is carried out.

Susceptibility to drugs is known to vary according to diet 'not only the diet on which the animal was reared but that of parents and grandparents' (Leitch, in Russell, 1948). Lubitz, Levine, and Fellers (1941) found that a diet containing 45% casein increased the resistance of male white rats to red squill so that only two out of ten died at a dose that killed seventeen out of nineteen on a low protein diet. Dieke and Richter (1945) by changing the diet of one strain of rats, altered their L.D. 50 from 44 mg. Antu per kilo to 640 mg. per kilo. Resistance to thiourea, the parent compound, was greatly lowered by an oatmeal diet (Landgrebe and Morgan, 1946).

Except for the rather special case studied by Byerrum (1946) I have found no information about the speed with which diet alters susceptibility;

but it seems unlikely that a few days of feeding on wheat would produce a very marked change in the resistance of a natural population. It is more probable that heterogeneity in their genetic composition is responsible for greater variation in susceptibility than any short term effect of diet (Ch. 2, Appendix 1).

Changes in body weight as a result of census baiting are unlikely to affect the numbers killed. Some rearrangement of the population is to be expected in relation to the new food supply, but if this is well distributed the consequences are not likely to be serious. New holes are quite often made in the earth near containers, but usually they appear to have been used for cover while the rats are eating the baits. Day-time activity, however, may be markedly less in well-fed populations, so that those receiving surplus baits may feed less during the day-time and not be as eager when they begin nocturnal feeding.

(ii) *Interval before poisoning*

If census baiting continues up to the time of poisoning a special set of conditions has been introduced which is not typical of that particular habitat. In certain tests census baiting has been continued during and after the day of direct poisoning. On a well-provisioned farm the difference between rats taking one kind of cereal from a P3 instead of another kind from a sack nearby is probably unimportant, but in other environments a purely experimental condition is produced.

The two most important and related factors which census baiting may eliminate—hunger and day-time activity—often start again within a day after the good feeding stops, and it seems probable that the population will quickly resume its normal feeding habits; though possibly some natural food accumulates through not being eaten during census baiting. An arbitrary gap of 3 days is generally allowed after a census before direct poisoning; but with prebaiting, the first baits are laid immediately. In the case of standard prebaiting with gaps (the 1–3–5 system, Ch. 10) the rats have 4 days between census and poisoning in which to resume their former habits, including 2 on which they receive some extra food in the form of prebait. Longer intervals than 3–4 days might be desirable but for the greater likelihood of changes in the environment and numbers of rats and for the inconvenience of prolonging a test.

(iii) *Checks on procedure*

The probability that the procedure normally adopted in census baiting does not influence the result of the poisoning is confirmed to some extent by the close correspondence between similar tests checked by other methods. The efficiency of zinc phosphide, and the effect of leaving out prebaiting, work out about the same when measured by census baiting, sight counts, hole blocking, or collecting live rats and corpses. Also the efficiency of a large-scale practical operation, rarely checked by census baiting, gave the same clearance as was to be expected from the more carefully measured trials (Ch. 10). Watson and Perry, in the Middle East, often started prebaiting at greater intervals than 3 days after the end of a census (Ch. 14).

(*b*) Choice of census bait

It is probably desirable to have one type of bait, which must therefore be attractive in nearly all environments. It should be available in standard form requiring little or no preparation before use and should not change or deteriorate with storage or under field conditions. It must be a food which the animals eat with little waste, do not hoard, but can remove from containers visited by large numbers of rats. It should be cheap, not too bulky, must generally differ from the bases to be used in poisoning, and must be edible under dry conditions. Not all of these requirements are likely to be met and in practice one or two other census baits must be available.

In the majority of our tests, dry whole English wheat has been the census bait but it is not ideal in all respects, and other foods were equally or more suitable on many occasions. Wheat is not a perfectly standard bait and a surprising difference was once found between the palatability of two samples (Table 39). Not all rats will take dry wheat readily, though individual exceptions are probably to be expected with any bait.

Watson and Perry found a moist bait necessary in the Middle East and were able to use a different soaked cereal from the one used for poisoning. In certain British environments where it was the only base known to be attractive, soaked wheat had to be used for both census and poisoning. Our evidence suggests that survivors do not avoid taking soaked wheat after its use with poison. Nor is there any case in which whole wheat was avoided after a poisoning with a wheat product such as flour or even after kibbled wheat (§ 17).

(*c*) Refusal to enter containers

In some environments rats are very reluctant to enter containers. Sometimes this may be through lack of an attractive bait, but Freeman's 'outside' rats (§ 6 (*b*)) which could be baited with dry wheat on the floor refused to feed from pipes. One such instance occurred in a buffer depot of white flour and sugar. The rats were living in wasteland outside, and thirteen pipes, baited with wheat, were examined daily from the eleventh to eighteenth days. Rat tracks in flour patches passed right in front of each pipe, but only about 50 g. was taken from inside one of them and a little scattered wheat from outside others. When wheat was placed outside the daily takes (g.) built up as follows: 75, 295, 400, 600 (no surplus), 730.

Similar difficulties were found both with pipes and P3s in certain food premises, at wheat ricks, in hedgerows bordering fields of standing wheat, and at an offal dump. At the latter place no wheat at all was taken from twenty-four pipes until the twenty-fifth day. Yet wheat placed in holes was taken readily.

It is to be supposed that with ample natural food rats move around less than they do when hungry. Starvation increases the activity of white rats (Hitchcock, 1928) and during token baiting at the experimental colony signs of activity were far more obvious than when food was adequate. Strongly marked food preferences and lack of hunger may not entirely account for the avoidance of containers, and it may be that new object reaction is unusually prolonged. However, the practical significance is that such rats cannot be census baited in pipes or P3s, so that where surface baiting is impracticable other methods (§ 3) have to be adopted.

(*d*) **Overcrowding**

The amount of bait taken from a census point should not be limited by the fact that some rats cannot get at it. If there is overcrowding or fighting some rats will be prevented from eating all they might, whereas in the reduced population after poisoning there will be less competition and the takes may not be comparable.

Unless in unusual situations, surface points seem able to accommodate very large numbers of rats, though fighting has been observed. In two different marked populations one rat repeatedly drove off another before it had been long at its food. However, the feeding times of one of these populations tended to be staggered (Table 20) and other studies show that there is ample time during the night for one individual to feed in the absence of the other (Fig. 13). The remarkable fact is that great concentrations of

TABLE 20. *Difference in feeding times of individual marked rats, 28 June 1941*

No.	Sex	*Body wt.* (*g.*)	*No. of visits per 5 min.*													
			–9.00		–9.30						–10.00 p.m.					
12	♂	120		*	1	1	–	–	1	1						
14	♀	405			1	7	2	6	4	1	–	1				
22	♀	380						*3*	*1*	*1*	*1*	–	–	*1*		
31	♂	115						1	2							
33	♀	405†								*1*	*1*	*1*				
25	♂	53								1	1	2	4			
32	♂	225									3	3	3			
9	♂	80	*	*							2	4	4	5	3	4
23	♀	330											1	*1*		
8	♂	175													1	–

* Uncounted number of visits. Italics show settled feeding (§ 6 (*a*)). Nos. 22 and 33 characteristically ate without running to cover. 8.10–8.45 and 10.00–10.20 p.m. only unmarked rats feeding. Double British Summer Time.

† On two previous occasions this rat attacked and drove off No. 14: once when the latter had just appeared, once when it had arrived first and begun feeding.

rats will feed together within a small space. Twenty-eight rats, total weight 5·4 kg., were once trapped while feeding simultaneously in a box 14×12×9 in. high. Other remarkable concentrations have been observed at the food-recording machine (Fig. 1). Here the pile of wheat (continuously replenished from a hopper) had a circumference of only a few inches, yet as much as 4 kg. wheat could be removed in one night, and on one occasion about 160 rats all finished their first meal in less than 2 hours. P3s with a glass back or top have been used during watching at the experimental colony and some rats while in occupation prevented others from entering. A population which had previously taken about 2 kg. per night from two trays failed to take more than 850 g. from two P3s (Table 21, first test).

Effects of overcrowding have been tested by starting out with one census point and adding bait to another one previously placed nearby. If the total take increases then it may be assumed that the first point was overcrowded and that the take was not proportional to the feeding capacity of the rats attempting to use it. Our observations suggest that large amounts can be taken from a single P3, but that on occasions overcrowding may limit the take below 500 g. per day (Table 21).

TABLE 21. *Effect of overcrowding at containers*

Takes of wheat (g.) on consecutive days in six tests	*Baiting points*
1,937 - 1,938 1,895 **2,092**	2 trays
Token baits for 5 days 715 **850** 820	2 P3
.. **175 - 175** 142 165	1 P3
200 230 296 167 - 168 **300**	1 P3 and 1 surf. pt. 6 ft. away
550 427 - 428 - 429 492	3 P3
568 503 **730** 642	3 P3 and 4 surface points
400 - 400⁺ **640** 580 - 580 497	1 P3
370 415	Tray 6 ft. away
655 650 **715**	1 P3 and tray
460 495 - 495 530 **580** 525 455	1 P3
707 855 825 **863**	2 P3, 3 ft. apart
635 **795** 625 775	Original P3
.. 2,030 1,885 **2,080**	1 surf. pt.
745 **1,190** 1,063 1,100	2 P3
1,230 1,197 1,208 1,455 1,900 **1,950** 1,788 1,760	3 P3
1,532 1,585 1,422 1,423-1,620 1,770 2,217 2,285 **2,290**	2 P3

Records read consecutively from one line to the next; hyphens show average takes; peak in heavy type.

Rats frequently sit and eat inside P3s but, though they generally remove wheat from pipes and eat it elsewhere, there is sometimes much crowding. This was one of the earliest observations and the policy was adopted of placing pipes in groups of two or more so that wheat would always be accessible from one of them. However, even in heavy infestations the rats may concentrate on one particular pipe until it is exhausted before they take much wheat from the next (Ch. 5). As the capacity of one pipe is fairly limited several are usually placed with not more than 400–600 g. in any one, and ill effects from overcrowding are probably rare.

In any experiment where the census points were overcrowded the original population will be underestimated. A successful poisoning will remove the effects of overcrowding and presumably enable the survivors to consume larger amounts of wheat than they could before. The actual kill will therefore be underestimated.

(*e*) Restricted census

The next problem is distributing the census points. It is probably desirable to have at least one within the normal foraging range of each rat; but this range varies widely according to habitat. In places rats may travel at least 100 yds. to and from a feeding point (§ 3 (*c*)), in others they may be highly localized, and it proved impossible to draw up general rules about spacing. Furthermore, sites must be free from interference and reasonably accessible to the worker, and their numbers are limited by the weight and bulk of containers and time taken to make many individual measurements.

On occasions where complete census baiting was impracticable a few stations only were dispersed through the habitat. Since a rather important consequence may follow from this procedure, it is worth stating the problem in some detail. Suppose that in a population of 100 rats only eighty are visiting the census baits. Let it be supposed that half the population is killed by poison thoroughly distributed over the entire area. Of the fifty survivors forty will be census baited animals, apart from random errors which should even out over a series of tests. A more important question is whether the first and second baitings are comparable; for if the reduced population pressure results in any change in the range of the survivors, a number of animals missed in the first census may be included in the second. If all the ten unbaited survivors in our example now extend their range to the bait points the number taking postbait becomes fifty instead of forty and the apparent kill falls to from 50 to 37%. The magnitude of this error will obviously vary according to the size of the sample; it will also be affected by the success of the poisoning and whether or not the animals fully baited in the second census had been partly baited before.

With a restricted number of dispersed census points, the tendency should thus be to underestimate the kill. It is therefore important, in testing a method which may not give good results, for the census to be complete. If it is not, the data may give only a minimum figure which does not prove the control method to have been unsound.

(*f*) Incomplete poison distribution

In the example just given it has been supposed that although the census points were limited, poison was distributed throughout the habitat; but it sometimes happens that some part of the infestation is not poisoned at all and the population density remains locally unchanged. A gradient in numbers is then formed which may result in movements into the treated area. As in the example discussed in § 4 (*b*) the postbait will not show the full effectiveness of the particular bait or poison being tested; but if the rats stay where they are, the census will give an unreliable estimate of the control achieved over the area as a whole. An excellent example is provided by the anomalous result of a test in Palestine (Ch. 14). Here both poisoning and census failed to cover the whole area; but had the poisoning been extended beyond the census area it is unlikely that serious bias would have been introduced.

(*g*) Reduced competition for food and water

Where the normal food supply is plentiful a rat will probably take as much wheat in its diet before the poisoning as afterwards; but where food is normally scarce the removal of most of the population leaves the survivors a better choice. An inadequate supply of water for the original rats may also prove sufficient for the survivors and enable them to take the postbait more freely. In addition, a number of corpses will prove edible for several days (but see § 15 (*c*)).

The extent of the error caused by reduced food pressure must depend very largely upon the attractiveness of the census bait in relation to both quality and quantity of alternative food. The method of determining drop in bait take by trapping out part of the population (Chitty, 1942) will not be reliable if the remaining individuals are thereby enabled to take a larger share of alternative foods. This difficulty may explain some of the anomalies which have since been encountered with this method.

(*h*) Growth and breeding

Food consumption increases with body weight, but within 2 or 3 weeks it is unlikely that normal changes in adult body weight are significant. Pregnant and lactating females require more food, but over a short interval the increase in food required is unlikely to be a large fraction of the total. The probable scale of the error was illustrated in an outdoor cage when one of eight rats gave birth to eight young, thus doubling the numbers. Food requirements increased only gradually and when the young were removed at the age of 4 weeks the average daily take fell only 60 g. from a peak of 286 g. (Table 18). Even in the unlikely event of all the adult females giving birth on the same day no change in food requirements comparable to that in numbers would have occurred within any 2–3 week period.

In this respect census baiting is better than any method which consists of measuring the numbers of the population without taking account of biomass. In a method such as counting individuals, droppings or tracks, there is an abrupt change when the young are weaned. In measuring bait take there is a slower transition since the young have indirectly been responsible for a proportion of the take since the day of conception.

Some error, however, is inevitable in a breeding colony. When the young are still in the nest they will escape the poisoning and may die of starvation. They will then have increased the difference between first and second census without having been included in the poisoning. On the other hand, if they survive, their postbait take will reduce the measure of success against the population actually at risk.

(*i*) Movements

Sudden large-scale movements are known to occur and must occasionally coincide with field tests. An invasion during the first census is not necessarily very serious and probably requires only a few extra days' baiting; but an increased take due to immigration during the second census may be indistinguishable from the normal build up. This source of error could invalidate any census method; but the likelihood of invasion is probably greater where bait is provided. It is not, of course, permissible to reject a test merely on the suspicion that immigration caused a high postbait.

Apart from abnormal or seasonable movements there may be an infiltration from any adjacent but untreated infestations. Critical tests should not be carried out under such circumstances, though it may be possible to interpret the results if a sufficiently careful study is made, particularly with the help of trace patches.

It is a popular belief that rats emigrate as a result of being poisoned. No evidence to support this belief has come from any of our tests. Evidence to the contrary is provided by the numerous tests of direct poisoning and, in the early days, of prebaiting with inefficient baits when the populations before and after treatment were identical or not much different. Furthermore, it would have been very obvious if there had been an increase in the rat population of neighbouring properties of the order of the decrease observed in our later field tests.

(*j*) Differential mortality

Susceptibility to poisons is not necessarily a constant in terms of body weight but may be more closely related to blood volume, or to metabolic rate, which varies geometrically with body weight (Kleiber, 1947). There are also various gross differences in susceptibility according to age, sex, &c. Consequently one possible effect of a poisoning is the selective removal of certain classes. Antu is markedly less toxic to young rats than to old (Dieke and Richter, 1945, 1946), the change coinciding with, but not being produced by puberty (Rogers, 1946). Young males may be more resistant than the older ones to barium carbonate (Ch. 2) and Hammett and Nowrey (1922) found some increase with age in resistance to arsenic. With these poisons, therefore, different numbers of rats (per 100 g. wheat take) may be present after poisoning than before.

There are also likely to be chance differences between original and residual rats in weight distribution and daily takes. Thus in an outdoor cage test, two large rats survived one much smaller one. By numbers the test killed 33%, but through random errors in census baiting the reduction appeared to be nil. Discrepancies of this sort are inevitable with small populations, for one large rat may eat 4 times as much as a small one; but such errors may be expected to even out in the larger populations.

Red squill and perhaps barium carbonate (Ch. 2) are more toxic for female white rats than for males. It is not known if differential mortality occurs in the field, but in the non-breeding state an abnormal sex ratio among the survivors should not greatly affect the census. The elimination or survival of an undue proportion of pregnant or lactating females would cause a rather greater discrepancy between estimates based on census baiting than on numbers.

(*k*) Illness

The speed of action of different poisons varies considerably in the laboratory. After coarse arsenic the rats lingered many days whereas all deaths from zinc phosphide occurred within 2 or 3 days. Animals which are going to die lose their appetite quickly, as was strikingly shown after our only field test of coarse arsenic. Rats disturbed the contents of the P3s for the first 4 days and left footprints elsewhere up to the sixth day but not afterwards. The postbait was 0, 33, 0 (for 9 days).

Rats which are going to recover from the inorganic poisons or Antu seem to get back their appetite fairly quickly. In four tests where census baiting was carried on throughout the poisoning its effects seem to have been overcome quickly (Table 22). In most tests, however, the rate of recovery of appetite cannot be distinguished from the normal build-up in take after interruption of the wheat supply. There are no obvious differences in these rates except after squill. In four tests of a poor sample (Table 51 (*c*)) peak postbait takes occurred on the fifth, sixth to eighth, ninth, and tenth days respectively. After squill in sugared rusk peaks were considerably earlier

TABLE 22. *Take (g.) of census bait before, during, and after direct poisoning in holes, &c., with commercial baits*

Poison	*Daily takes (g.) of wheat*							
	Before		*During*	*After*				
Phosphorus . .	4,565	5,115	3,295	2,740	3,040	2,815	..	..
" . .	365	400	305	275	285	290	255	300
" . .	180	173	156	158	158	122	120	..
Zinc phosphide .	210	100	80	40	125	75	..	..

and there was no evidence of a rising take after the second to fifth day of the 6-day postbaits. After squill extract, a peak occurred on the seventh day in one test whereas in two others there was an early peak and then a drop.

After red squill there is thus a suggestion that illness has more important effects on the wheat take. The evidence is far from satisfactory because of the rather longer postbaiting carried out, with the consequence that earlier takes may have been exceeded by chance, or because of immigration, &c. However, the symptoms of red squill poisoning are so prolonged and drastic that it seems reasonable to require a longer (or later) census than after other poisons. Postbaits covering only the first 5 days after poisoning may thus exaggerate the success achieved. On the other hand, if a longer period is considered, no strict comparison can be made with the inorganic poisons for which there may not be equally long postbaits. Moreover, the peak take may be high soon after inorganic poisoning because the rats are hungry after their illness; hence the kill will be underestimated. This risk must be taken, for it would be a worse error to take an average which included the first day or so when the rats are ill. Other irregularities in early postbait takes also suggest that the first 2 days should not be used as an index of population size.

(*l*) Discussion

A test of the efficiency of control originally consisted of surplus prebaiting, poisoning, and postbaiting in containers, often using the same base throughout. It was then found that survivors might be shy of taking the postbait, for which a different base therefore had to be used. Yet the postbait had to be comparable with the prebait, so that two different baits were now needed: (1) to measure the population, (2) to accustom the rats to the base to be used for poisoning. Operation (1) was now called census baiting, the other was prebaiting for poisoning or simply prebaiting.

It then became apparent that if there were insufficient containers to attract all the rats on an area, groups of animals would be left at a higher concentration than those round the containers. If these untreated animals extended their range the postbait would be sampling a different population from that of the first census; if they stayed where they were, the results, as estimated from the containers, would not be a fair sample for the area as a whole. With most small, isolated populations there is little chance of these errors, but the larger the area the greater the practical difficulties of covering it all. Moreover, since containers might perhaps be avoided for a while after the poisoning it seemed best to make census baiting an entirely independent operation from control.

In order to introduce the system subsequently adopted in our own work a series of field tests was examined (§ 5). Their results suggested that within 5 days a population of rats could change its normal feeding habits and take census baits of wheat at an approximately constant level. An apparently similar equilibrium was also reached within 5 days after poison had been distributed at other sites throughout the environment. In order to make general use of these observations it was necessary to test the assumption that valid equilibria had been reached and that the two were comparable. For this purpose evidence about rats' feeding behaviour was examined and it was concluded that this working hypothesis did not cover all cases. There were sometimes errors due to waste, hoarding, &c., and others in the actual measuring; but they seemed less important than those which exaggerated or unduly reduced the difference between first and second census. These errors were of two kinds: (1) chance differences between the original and residual populations—in weight, sex, or breeding condition—which might affect the interpretation of individual tests but should even out over a series, (2) factors in the environment or the design of an experiment which biased the comparison between the two populations. Depending on which census was affected the apparent reduction of the population would be better or worse than the true effect.

Errors due to the use of insufficient census points tend to give an underestimate of the true efficiency of a poison, but the duration of the experiment may affect results either way. Thus an incomplete build-up in the first census will reduce its difference from the second, which starts with rats already familiar with the baiting points. There will be a tendency to overestimate success if the postbait covers only the time the rats are ill, but to underestimate it by including high takes immediately after recovery or those which occur by chance the longer the postbait continues.

The amount of movement into the area being studied depends largely upon a wise choice of site; but it is not always possible to treat a large area as a unit or to find small isolated colonies. A steady influx of rats which indefinitely prolongs the build-up in the first census will lower the apparent kill; for while there may not be time to census them fully it will generally be possible to include them in the more widespread operation of poisoning. An influx in the second census will again lower the apparent kill. Emigration is unlikely while additional food is being provided in the form of census bait or prebait and there is no evidence that it occurs as a result of poisoning. However, with a long interval between censuses, movements may have quite unpredictable effects. It is also desirable to complete both baitings quickly because of natural changes in the population, though these are likely to be small in comparison with the effects of most methods of control. Occasional accidents must be expected to upset the comparison between first and second censuses and some form of paired testing is desirable though seldom practicable (§ 19). Little precision can be expected in a census of a small population.

So far, then, in an experiment with a sufficiently long postbait, most tendencies to bias the results are towards giving an underestimate of the reduction. This may also be the case if the water supply is inadequate for the original population but enough for the survivors. However, if food is scarce, this tendency may be reversed. Underfed rats will have large appetites, and unless the survivors again become equally hungry in the interval between two censuses they will presumably take less postbait per unit body weight. Also there will be at least as much natural food present after the poisoning as before when there were more rats to share it. However, it may be supposed that where the rats are hungry their food often lacks quality as well as quantity and that wheat would usually be eaten in preference to other foods. Beyond this it is impossible to speculate; but evidently some reservation must be made about the complete validity of results where food pressure is extreme. Thus if the apparent success in these environments (as measured by census baiting) is consistently better than in others it must not necessarily be assumed that such rats are easier to kill.

Apart from this perhaps remote likelihood the chief danger of census baiting as a test of the relative efficiency of control is that, except with red squill, the poorer results may sometimes not be quite as bad as they appear. Thus it may not always be possible to state exact quantitative differences; but it is unlikely that big differences between two series of standard tests can be explained by faults in the census technique.

If one method gives a slightly greater reduction than another there is probably no noticeable difference in immediate effect (except where one gives complete elimination). It is also of no immediate consequence whether any persistent damage is done by large rats or a greater number of small ones. From the short-term point of view, therefore, of making quick and simple comparisons of efficiency it is enough to group results rather broadly according to the total reduction achieved. However, this procedure does not take account of any difference in long-term effect. For example, a poison with an age- or sex-specific action which left a preponderance of females or young would be of least permanent value.

Neglecting these complications, the value of a control measure must ultimately be assessed, not in terms of the past as is usually done, but upon the permanence of the relief. Thus, while there may be little to choose between kills of 50% and 60% it might be claimed that a 90% kill is half as efficient as one of 95% because twice the proportion of survivors is left. However, a number of factors of population growth are involved which are difficult to study in natural communities. Laboratory studies of other species show that certain populations at very low densities are disproportionately slow in increasing their numbers. Emlen, Stokes, and Winsor (1948) have suggested that this may also be true of severely reduced rat populations. There is obviously a chance that recovery from very low numbers will be greatly affected by natural or accidental death or predation and by the reduced opportunities for mating. As far as may be judged, therefore, slight increases in the higher ranges of efficiency are probably of great significance in the maintenance of satisfactory rat control and should be given appropriate weight in considering the relative merits of two systems.

(*m*) Summary of procedure for field testing

(i) *Choice of infestation*

The most useful size of colony is probably one of at least twenty rats and not more than 100, i.e. with a daily capacity for wheat of 500–2,500 g. Colonies taking 200–500 g. wheat may be used, but estimates of the results are more variable. Still smaller colonies are seldom profitable for standard tests (§ 8 (*j*)). Very large infestations, certainly those of over 200 rats, should be studied by other methods (§ 3) or by a restricted census (§ 8 (*e*)). A careful survey must be made of the extent of the infestation which, in critical tests, must be isolated (§ 8 (*i*)). The possibility of interference by human beings or other animals must be guarded against from the start (§ 6 (*a*)).

(ii) *Census points*

Baiting indoors should, where possible, be done on the surface or in shallow trays because of the shorter period of 'lag' (§ 4 (*a*)) and greater number of rats which can feed freely (§ 8 (*d*)). Precautions must be taken against wet weather and desiccation (§ 6 (*a*)). Containers, often in groups, should be used in sufficient numbers so that no more than 500 g. per day need be taken from any one (Table 21). There is likely to be less error from using too many stations than too few.

(iii) *Census bait*

Whole dry English wheat is recommended as the standard census bait, but under dry conditions it is essential to use a moist bait (§ 7 (*d*)) and in certain environments soaked wheat must be used (§§ 6 (*b*), 8 (*b*)) or other baits such as bread mash. The same stock of bait must be used throughout (Tables 38–39) and must nearly always be a different kind from that used with poison (§ 4 (*b*) (iii)). Supplies should be renewed so that they are kept free from rejected residues (§ 6 (*a*) (ii)) and a small but adequate surplus must always be available.

(iv) *Census*

Time may be saved by liberal baiting when the stations are first placed. Seven to 10 days' lag must be anticipated with containers; there will then typically be a daily build-up in take. A standard period of about five daily measurements is desirable; but as behaviour varies between colonies the criterion may be adopted of continuing baiting until a steady level of feeding has been maintained for 3 days (§ 5 (*b*)).

(v) *Control treatment*

When a prebaiting system is being tested, the first prebaits are laid on the day of the final measurement in census 1. With direct poisoning a gap of 3 days is left. These intervals are arbitrary (§ 8 (*a*) (ii)). Where a poison or bait base is being tested the results must not be affected by other factors such as the baiting system or an insufficient number or size of poison baits. Any treatment, whether by gas, traps, or poison should cover the entire area round the census points and extend some distance beyond if the infestation is part of a wider one which cannot be measured as a unit (§ 8 (*f*) and (*i*)). Baits should be put in all suitable places other than the containers used for census baiting.

(vi) *Postbait* (*census 2*)

The postbait may start on the first day after poisoning; but the takes on the first 2 days are seldom useful as the rats' appetites may be reduced by the poisoning (§ 8 (*k*)) or increased by lack of food, or hoarding may occur (Table 8). Additional checks on the results are recommended (§§ 3, 8 (*a*) (iii)).

9. RELATION OF CENSUS BAITING TO NUMBERS

As shown in Chapter 6 the daily requirement of the average rat in a rick is 24 g. wheat. No allowance is made for pregnant and more particularly lactating females, which may require 3 times as much as before conception (§ 7 (*b*)). In the non-breeding, winter populations outside the ricks the average weight was greater and food requirements were 31·4 g. per rat per day.

Because of the error of these averages and since each population may differ in its weight distribution and breeding condition it would be unwise, for these reasons alone, to expect accuracy in estimating the numbers of rats eating census baits. However, even in populations with exceptional appetites or numbers of lactating females the order of magnitude should be 2–4 rats per 100 g. wheat provided little else is being eaten. This estimate will be low in proportion to the energy derived from other foods and some attempts have been made to see how thoroughly census baiting attracts the rats.

With a small population of known feeding habits it should be possible to measure the total daily take of normal food to find how much is still being eaten during census baiting. However, in an environment sufficiently simple for such an experiment it is not as difficult to get the rats fully baited, as it is in the more complex infestations. It is seldom possible to measure all the food available in a large area, though at one farm an attempt was made to sample it. The rats were largely living on crushed oats which at this particular period in the war was one of the few foods available for a herd of fifty cows.

The greatest concentration of rats lived in and around a barn where sacked oats were stored before being crushed and piled on a concrete floor. Other rats mainly fed in three cowsheds, particularly in one where crushed oats were stored in an open bin from which the mangers were filled. It was never possible to measure the food in the manger as it was mixed with hay; but on occasions the other three sources of food were reduced to a few pounds each. This happened first on 1/ May 1941 except that there were several sacks of oats in the barn in addition to that spilled around them. During the afternoon the barn was carefully swept out and the piles of food were weighed here and in the cowshed, which was already clean and tidy. At 5 a.m. the next day, just before milking time, every building on the farm was visited and the rats were counted as they ran away in the beam of a flashlight or were heard.

In the barn the rats had not contented themselves with eating the crushed oats but had torn open three sacks nearby so that only a minimum figure can be given for the amount eaten.

An exactly similar test was carried out on 10/11 May when census baiting was in its seventh night, and the barn contained only a small pile of crushed oats. The difference between the amount of normal food eaten on the two occasions may be regarded as a sample of the thoroughness of the pre-baiting. There was also a well-marked drop in the numbers of rats seen and heard, indicating that instead of using their normal feeding sites they had been intercepted closer to their holes (Table 23).

TABLE 23. *Effects of census baiting on the take of natural food and on the activity of rats*

May 1942:		*/2*	*/11*	*/13*
			g. food eaten	
Natural food	Barn	> 1,530	340	..
	Cowshed	710	200	..
	Both	> 2,240	540	..
Census bait	Barn	0	2,280	2,670
	Cowshed	0	1,200	1,510
	Elsewhere	0	1,055	935
	Total	..	4,535	5,115
Total known take of food		..	5,075	..
No. of rats seen and heard		26	8	..
% reduction in sample				
Take of natural food	Barn		> 78	
	Cowshed		72	
Rats seen and heard			69	

The take of census bait on 10/11 May, the second occasion, was 4,535 g., giving a total known consumption of 5,075 g. cereal. In addition some food must have been taken from places where it could not be measured, though it is believed that by this time the rats had mainly ceased to feed from these other sources. This belief is based on the distribution of rats at the morning counts and on the fact that the take from the census points ceased to rise after this date except near the barn and the one cowshed. Two nights later the total take of census bait rose another 580 g. making a total of 5,115 g., which suggests that the entire population had been drawn away from its abundant natural food. Though it would be rash to claim that census baiting was as effective as these figures suggest it was evidently meeting a very high proportion of the total daily requirements.

In other tests we know from watching, from stomach examinations, and from trapping out part of a population that the rats have continued to eat a certain amount of their normal food and that this proportion varies according to the habitat and the type of census bait.

On other occasions more rats were picked up after a poisoning than were estimated to have been killed. In some cases too few census points had been used, accidentally or by design, or the census bait was obviously unattractive. Fifteen such tests may be disregarded; all others for which there are data about corpses have been plotted in Fig. 9. It is at once obvious from this figure that, in the great majority of tests with all poisons, the number of corpses recovered was well below the numbers estimated to have been killed on the assumption that there were 2–4 rats per 100 g. reduction in take. There is a great deal of variability with each poison as is to be expected from the wide variety of habitats alone. The uselessness of corpses as an indication of success is clearly shown by the many large reductions accompanied by small numbers picked up.

The highest proportion of corpses was recorded after zinc phosphide treatments and some of the lowest after arsenic. In a paired trial of these two poisons (Table 27) zinc phosphide yielded twenty-three corpses, arsenic none. Such results are partly responsible for the mistrust with which arsenic is sometimes viewed, though, as in this test, it killed most of the rats.

Comparatively few corpses were recovered after Antu, four being the greatest number in all but one of the six largest tests. In the paired test in Table 2 there were thirteen corpses from the section treated with Antu and thirty from that treated with zinc phosphide.

Fig. 9 shows that there were several cases in which relatively high numbers of corpses were recovered after squill extract and there is at least a suggestion that more rats died on the surface than after the powder had been used. There are no data for barium carbonate and no obvious differences in the corpse-recovery rate according to the type of bait base.

Thus the evidence from corpse recoveries is seldom against the possibility that a reasonable estimate may be made of the numbers of rats killed. It is also obvious, especially with arsenic and Antu, that this type of evidence is not by itself any proof of the completeness of the census. With zinc phosphide, however, it might be expected that if census baiting had

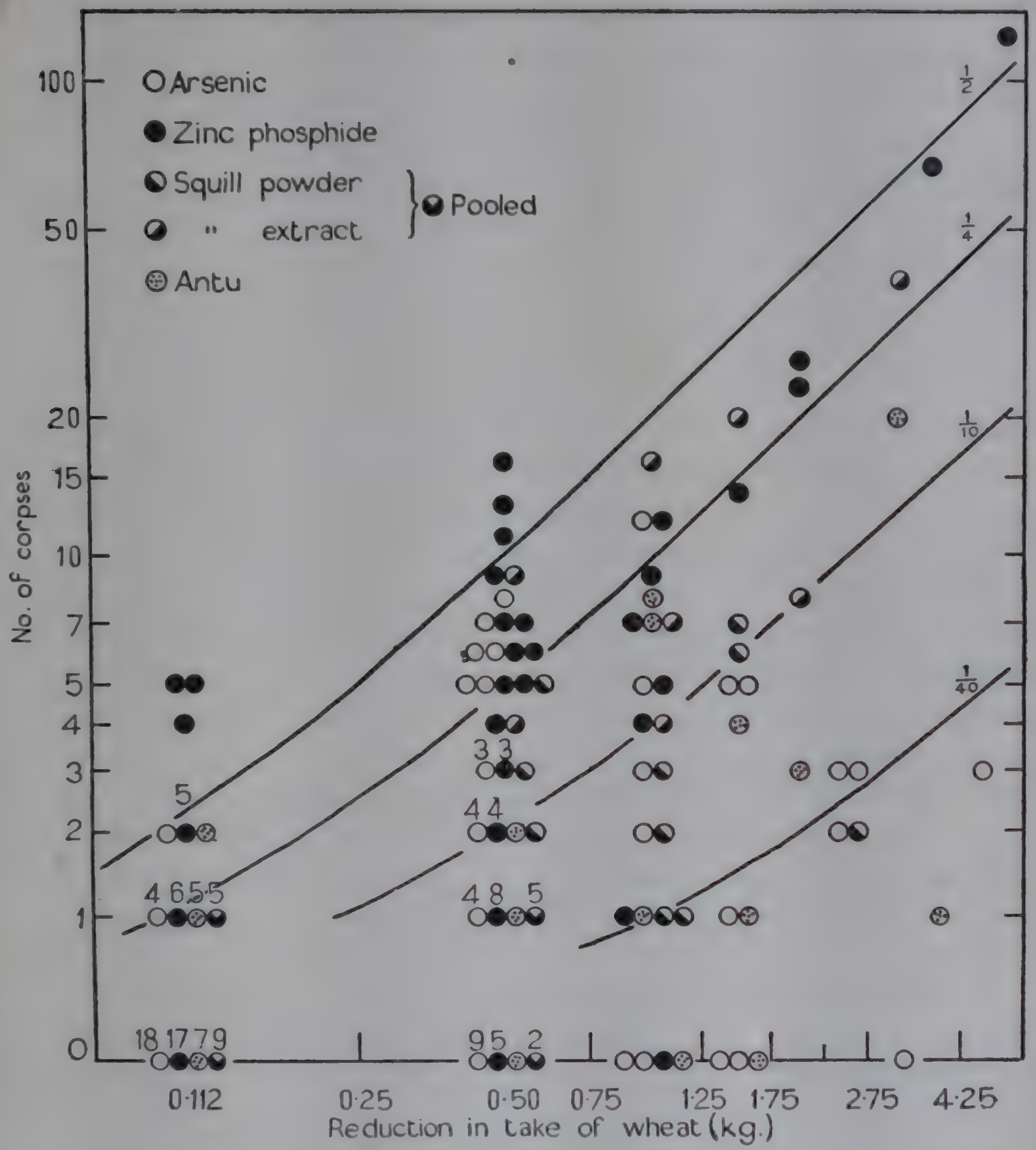

FIG. 9. Number of corpses picked up relative to the reduction in wheat take. Log reduction (kg.) is plotted against log (number of corpses + 1). Tests have been grouped according to the reduction as follows: 0·025–, 0·251–, 0·751–, 1·251–1·750 kg. and plotted at the mid-class intervals. Tests with > 1·75 kg. reduction have been plotted individually. Each unnumbered symbol represents one test; in other cases the numbers above the symbols show the number of tests. Curves show the maximum expected number of corpses if $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{10}$, or $\frac{1}{40}$ were recovered from a fully census baited population with an average take of 25 g. per rat. *Example.* The following numbers of corpses were recovered in those tests with zinc phosphide (●) which, on the average, reduced the census by 0·5 kg.: 0 (5 tests), 1 (8), 2 (4), 3 (3), 4 (1), 5 (2), . . . 16 (1 test). Five to fifteen corpses are to be expected from tests in this group if half the corpses are recovered, i.e.

$$\frac{1}{2}\left[\frac{250}{25},\dots\frac{750}{25}\right].$$

supplied only a small proportion of the total food there would frequently have been discrepant numbers of corpses.

We may conclude that with enough care it is possible to bait a population of rats so thoroughly that they take comparatively little of their normal food. As a rule, however, even when census baits are eaten in constant daily amounts, they are likely to form only part of the total intake, the proportion varying according to the habitat. Hence peak takes may represent different numbers of rats, though the correct order of magnitude will probably be 2–4 rats per 100 g. take of wheat.

10. SUMMARY OF PART I

1. Quantitative methods of studying wild mammal populations have been developed for various species; those which can be applied to *R. norvegicus* are reviewed. Some of the methods were used in the war-time research by the Bureau and their usefulness might be developed, especially by those concerned with large-scale operational research.
2. The major problem was to find a standard index of the abundance of natural populations so that control methods could be tested objectively and economically. A baiting method seemed the most satisfactory, but much work had to be undertaken to find out if any bias was introduced into the estimate of results.
3. These researches led to a study of the response of the brown rat when baited under various conditions of shortage or abundance of natural food, shortage of water, overcrowding, artificially reduced numbers, during convalescence after poisoning, and during periods of reproduction or rapid growth.
4. It was concluded that the surplus baiting method, if carried out with specified safeguards, was likely to give an unbiased measure or in some cases (other than those with red squill) a slight underestimate of the kill.
5. Although primarily used as an index of relative abundance, the surplus baiting method probably gave a reasonable indication of population size.

PART 2. RESPONSE TO BAITS USED IN CONTROL

11. THE COMPARATIVE EFFICIENCY OF THE POISONS

Information on the efficiency of poisons in the field is complicated by the fact that they have all been used under a wide variety of conditions. It is obviously impossible to compare the efficiency of one poison with another if the effects of other variables are too great. It is, of course, desirable to test each poison under all conditions, but there are practical limits to the amount of data which can be collected. The relative efficiency of the poisons must therefore be judged from results obtained under roughly similar conditions.

(*a*) Variables other than the poison

(i) *Control methods and environment*

The toxicity of a poison is expressed in terms of its lethal effect on laboratory rats of a certain stock kept under uniform environmental and dietary conditions. The poison is given in the same type and amount of base after a fixed interval since any previous meal; sex and weight ranges of the rats are usually the same in comparative tests. The object of this standardization is to obtain an arbitrary figure for toxicity (L.D. 50) with the least range of error for the limited number of rats usually available for bioassay (Ch. 2).

The efficiency of a poison against wild rats can at present be determined only from its lethal effect in the field since many other factors besides toxicity are involved. If these other factors varied each time, too many tests would be required to compare the efficiency of one poison with another. Since a different population must generally be used for each test certain variables cannot be eliminated, e.g. sex and weight composition, resistance to poison, and amount and type of food in the stomach. Variations in the baiting system, the bait base, the concentration of the poison and the amount of previous poisoning can be reduced to some extent; but environmental conditions are bound to vary. We must now decide which tests provide the most comparable data about the efficiency of different poisons.

Baiting system. Results are extremely variable when prebaiting is omitted or carried on insufficiently long. The 1--3–5 system of hole baiting (Ch. 10) or several days' continuous token baiting at a more limited number of surface sites are reliable methods; but no others can at present be regarded as fair tests of a poison's potentialities. Tests with surplus prebaiting or in containers must also be considered separately.

Base. Poisons cannot generally be tested in the field apart from the base; yet there is often none which is suitable for every infestation or poison. Squill and barium carbonate, for example, should be used only with wet bases (Ch. 7) whereas zinc phosphide is seldom used with bread mash. Much of the evidence in favour of arsenic comes from its use with soaked wheat, a highly acceptable base. The variety of bases thus limits the amount of directly comparable evidence, though in many habitats all the standard bases are probably equally good. Poisons used with bases such as plain flour or dry rusk must be regarded as tests of the bait as a whole and not of the poison only.

Concentration of poison. Within certain limits, differences in concentration probably have little effect; e.g. 10%, 5%, and, in rusk, 2% zinc phosphide give similar kills and may be pooled as evidence of this poison's efficiency. A lower concentration is generally adequate in wet baits because intake is higher than when dry baits are used (Ch. 7).

Previous use of poison. Certain rats which have survived a poison bait will subsequently refuse either the plain base or the poison in another base; if either was used twice within 6 months the second test must be disregarded.

'Special' habitats. In designing a series of tests to compare two poisons the types of habitat should preferably be restricted unless several tests can be made in each; but when tests have been done wherever possible it is not easy to decide which to use. The alternative to the variability to be expected from comparing results in all habitat types is to make an arbitrary and possibly false selection of evidence. It has therefore seemed best to include all measured tests except those carried out on sewer rats and in the Middle East. However, it must be borne in mind that the factors limiting the efficiency of two poisons may differ according to the environment. A poison tested only in grocers' shops might give different results from another tested in hedgerows (the effect of poison and habitat thus being indistinguishable) and while zinc phosphide and barium carbonate may be equally good in sewers, the lower toxicity of the latter may limit its usefulness above ground.

(ii) *Census methods*

Having decided which tests were least affected by peculiarities in the baiting technique, the population size and adequacy of the census methods must be examined. There should be a minimum of 5 days in each census, preferably with each day's take measured separately; but if all tests are rejected which fail to conform to later criteria the data on some poisons become very meagre. Only the following have therefore been excluded except where stated to the contrary: (1) where populations took less than 100 g. census wheat at the peak (the errors of measurement and chances of random error being too large), (2) where census and poison baiting were done in the same container, (3) where the same base was used for poisoning and postbait (except with whole wheat), (4) where census baiting was too short.

It is particularly difficult to define this last category. Consider, for example, the evidence in Table 24 about arsenic No. 31 on soaked wheat. In neither test is the first census sufficiently long; but while this is relatively unimportant in R.C.T. 1502 (because of the low postbait take), the measure of success in R.C.T. 1514 might have been better or worse if the first census had continued for 5 days. Yet both these tests or neither must be used, for by rejecting the less precise results we tend to eliminate those

TABLE 24

R.C.T. No.	Takes of wheat (g.)						
	Census 1		Census 2				% reduction
1502	1,150	1,100	0 35	40	40	..	96
1514	735+	1,035	token	110	100	125	88

which were less successful. Thus there are many cases where 100% success may be correctly claimed however inaccurate the initial census; but where several rats are left the success is unknown and the test must be rejected. In order to get an unbiased measure of a poison's efficiency this error must be avoided, and if R.C.T. 1502 is used then R.C.T. 1514 must also be included. No test has been omitted on the grounds that reinvasion might have affected the postbait; one test only has been so interpreted (p. 181).

A successful test is arbitrarily defined as one in which there is a reduction of at least 85% in the wheat take, or numbers of rats seen, or in those alive in a corn-rick. The reduction in wheat take is expressed as the per cent. difference between peak takes. The first peak is obtained from the last three measurements before poisoning (any average counting as a single measurement); the second peak is obtained from the takes on the third to fifth days afterwards.

(*b*) **Arsenic**

(i) *Early tests*

Fairly extensive data are available about baits with 5% fine arsenic, but so many other factors were involved in these early tests that few details will be quoted. Most tests were with whole wheat and 5% arsenic, in some cases stuck on with 2% oil after prebaiting with plain wheat. The oiled bait was often poorly accepted, perhaps partly because of the change to oil on the night of poisoning; but it was discovered later that the concentration of poison was, in any case, too low, except perhaps in wet bases. Fine arsenic was next used at 10%, especially with kibbled (broken) wheat which provides a larger surface area with better adhesive properties.

Until May 1942 census baiting and poisoning had, with a few exceptions, been done in the same containers. However, evidence was accumulating that pipes and P3s were not satisfactory for poison baiting large infestations, and the most probable reason was the comparatively small number of such baiting points which had been used. Before the need for more containers was realized new factors altered the situation and neither the efficiency of 10% fine arsenic nor of containers was thoroughly investigated. The introduction of hole baiting in May 1942 so simplified the problem of rat control that containers were seldom afterwards used for poisoning except in urban areas. Furthermore in June 1942 the original fine arsenic (sample 1) disappeared from the market before more than one standard test had been carried out on the new system. Other arsenics and zinc phosphide then became available and much of the summer and autumn of 1942 was taken up in testing these new supplies.

(ii) *Refined Canadian arsenic*

The medium grade of arsenic (refined Canadian, samples 5, 24, 31) which had replaced the fine, was not sufficiently toxic for use at 10% in sugar-meal. One good result was obtained, but there were two measured failures (Table 52 (*a*)) as well as one which was not measured, but left many rats, and another on a different baiting system which was probably not responsible for the poor result. At 15% in sugar-meal or at 10% on soaked wheat there is adequate proof of the high efficiency of Canadian refined arsenic and only two serious failures occurred with these baits. One test was in the Oxford market, a difficult environment discussed in § 16; the other was in a knacker's yard, where the apparent kill of only 32% was exceptional for any poison.

Flour without sugar was proved to be an unsatisfactory base (Table 52 (*b*)), but there are few data for other bases and different types of evidence must be examined. To the evidence about arsenic in rusk may be added two successes by methods which are not generally reliable: a single day's prebaiting reduced a dump population by 89% (Table 27) and direct poisoning completely eliminated some riverside rats (§ 12). Other tests with reduced prebaiting were less successful in ricks, but included kills ranging up to 86% (Ch. 10, Table 1). There were several successes and few failures with unmeasured populations and there is every reason to believe that a series of standard tests would have given results as good as those with arsenic in other bases. Bread mash was used on several occasions in P3s or pipes or at a few surface points. Table 55 (*a*) includes all tests where population size was determined by census baiting at the site of the poisoning or can be approximately stated from the take of surplus prebait or poison bait (Ch. 7). Extremely good results were obtained in most cases.

(iii) *Other grades*

In June 1942, a fine but crude arsenic, No. 19, became available. Since there was evidence that it was unpalatable to white rats in the laboratory and to three small colonies of wild rats, no properly conducted field trials were attempted. In November 1942 sample No. 20, an arsenic from Russian sources, proved palatable to white rats and successful in the outdoor cages. Field evidence was inconclusive, partly through faults in methods of testing, and Russian arsenic was not recommended while there were adequate supplies of Canadian refined.

By August 1943 research procedure had improved and several properly measured tests were done on a medium grade of arsenic (No. 26) from U.S.A. This sample was considerably purer than others, which may account for the very high level of success in the nine field trials.

One test with coarse arsenic was highly satisfactory despite the laboratory proof of its lower toxicity (last test in Table 51 (*a*)).

(*c*) **Zinc phosphide**

Our first recommendation for the use of zinc phosphide was principally based on ten trials carried out by Middleton in heavily infested rural habitats. Many of these important tests were carried out before census baiting had become the established procedure. Also the distance of the infestations from Oxford and the need for speed in making recommendations, made it desirable to try other methods of testing efficiency. Dusk counts, hole blocking, and general observations were made after each poisoning but not before. While these tests yielded the type of information then urgently needed they cannot be used in a quantitative comparison with other poisons. There was no doubt about the success of the treatments, however, since few rats remained from large populations (the total number of corpses recovered was 494). These tests were completed by the end of July 1942, four more tests were done later that year (Table 52 (*b*)) but few census baited tests were carried out for some time. Instead, attention was given to the use of zinc phosphide in corn-ricks and practical block control, where evidence of high efficiency was again obtained (Ch. 10). Table 53 summarizes the results of those tests which were done on the 1–3–5 system in ricks. Zinc phosphide also gave good results in the Middle East (Ch. 14), and in 1944–5 further evidence about it was obtained from tests with a lower concentration (2% in rusk) or with 3-week-old poison baits (Table 51 (*b*)).

Zinc phosphide was proved suitable with all the standard bases except bread mash, which was not fully tested. Unsatisfactory results were obtained when plain flour was used instead of sugar-meal (Table 52 (*b*)). Otherwise failures were extremely rare; but there were two notable exceptions: in the Oxford market with surplus prebaiting (Table 43) and at an offal dump. (This latter treatment was not checked by census baiting for reasons given in § 8 (*c*).)

(*d*) Red squill powder

(i) *Normal toxicity*

Squill powder was first tested in February and March 1941 and this evidence—of high but largely unmeasured mortality—need not be quoted. No further tests were carried out until 1943 when new supplies became available (Table 51 (*c*)). Seven tests were done in rural areas using rusk and the 1–3–5 system. Three results were less than 85% successful and as it was thought that this might have been because of the poison's bitter taste a series of trials was carried out using rusk with 10% sugar. These results were even worse and it seemed that the addition of sugar had decreased efficiency. This is difficult to believe, however, and it seems more probable that the results with sugar may have been due to chance because of a dangerously low concentration of poison. Six tests in containers where squill was used at 15% or in a wetter base were more successful and suggest that there may have been a greater intake of poison with these formulae (Ch. 7).

(ii) *Low toxicity*

Certain samples of squill powder were not recommended for field use if the L.D. 50 was as high as 800 mg. per kilo (Ch. 2). In order to test the validity of this advice four field trials were carried out with sample S 2 M.I., having an L.D. 50 of $>$ 1,250 mg. per kilo and four with sample S 6, L.D. 50 $=$ 857 mg. per kilo. Ten per cent. squill powder was used in 1:1 rusk on the 1–3–5 system (Table 51 (*c*)).

Sample S 2 M.I. One test was measured by a restricted census only and in all cases an unusually long postbait was carried out to allow for the complete recovery of appetite (§ 8 (*k*)). Both procedures tend to reduce the measure of success, but there can be little doubt that this powder was unsuitable for field use.

Sample S 6. Two of the four results were good and the other two were no worse than some of those with previous samples. Here, evidently, census baiting was not sufficiently accurate to establish any difference between the normal and low toxicity powders when tested in separate series.

(*e*) **Red squill extract**

Field work on alcoholic extracts of red squill started in the summer of 1943; rusk was the base throughout (Tables 51 (*d*), 52 (*c*)). Several tests using containers on small populations showed that Freeman's laboratory extracts (Ch. 2) were satisfactorily accepted and gave reasonable kills. Of four further tests, using these extracts on the 1–3–5 system, three were also successful, one was not. These concentrations were equivalent to 10% powder, but the commercial extracts were at first used at 5% with poor results. Later tests with the commercial extracts at 10% powder equivalent were highly successful, even in ricks. On another occasion three wheat ricks were prebaited for 2 days without gaps and poisoned on the third. At threshing there were six dead rats only in a population of about thirty, a result which seems too poor to be attributed solely to that prebaiting method.

In two outdoor cage tests surprisingly good results had been obtained with 0·5% powder equivalent. A concentration of 0·5% proved useless in a field test and no doubt the lack of alternative food in the cages had raised false hopes. One test with 2% was successful, even without prebaiting (§ 12 (*a*)).

(*f*) **Barium carbonate**

Field evidence about barium carbonate is unsatisfactory. No tests were done on the 1–3–5 system and nearly all populations were small. The first two tests in Table 55 (*b*) were done at surface points, the rest in containers. Because of the low toxicity of barium carbonate it must be used in a wet base and at a fairly high concentration. Bread mash was used in all tests and was satisfactorily accepted with 20% poison. Two tests with 10% and one (in a sewer) with 25% gave results of the same order, so that the most efficient strength remains to be found. Precipitated barium carbonate and the natural product, witherite, gave about the same rather unreliable results.

Two poisonings were also done at ricks by prebaiting with bread mash for several days at one or two sites per rick; but this method gives variable results even with arsenic and zinc phosphide (Ch. 10). On one occasion only nine rats were found dead at threshing, out of a total adult population of thirty-seven; but another rick population was successfully controlled after surplus prebaiting with bread mash. These rats took 4·4 kg. bread mash before poisoning and only 160 g. after. This rick was not threshed at the time but the apparent success was confirmed by a sight count in which no rats were seen, and by the scarcity of tracks in the snow. Bread mash and 20% barium carbonate was extensively used in the M.O.F.'s sewer rat campaigns and seems to have given results similar to those with arsenic and zinc phosphide (Ch. 12). From its low toxicity and the several failures barium carbonate seems less good than some poisons; but it is capable of giving 100% kills and needs to be tested by the improved methods which have grown up since we ceased to pay it much attention.

(*g*) **Compounds of Thiourea**

(i) *Alpha-naphthylthiourea*

With Antu, little attempt was made to establish its efficiency after prebaiting as there was already a sufficient choice of good poisons for the limited number of bases available during the war. Preliminary tests proved that the samples were toxic and palatable, a procedure most economically carried out in containers and with small populations. Other tests on the 1–3–5 system were highly successful (Tables 51 (*e*), 52 (*d*)) except that the first gave a poor kill with Antu at 1%, and in the Oxford market results were no more reliable than with other poisons. It was hoped to duplicate the claims, which could not at that time be checked, that Antu was efficient without prebaiting (Table 26 and Emlen and Stokes, 1947) and a series of standard tests was carried out. These showed that consistently high kills could not be obtained, at least with rusk or sugar-meal (Tables 5, 54 (*b*)).

(ii) *2-biphenylthiourea*

A sample of this compound, similar to Antu, was tested in a butcher's and an ironmonger's. In a test in which it was given at 2% on soaked wheat, the entire 140 g. poison bait was taken, which was 36% of the peak take of prebait, an unusually large proportion (Ch. 7). No rats were killed. In the other test a 5% mixture was given in rusk. One hundred and thirty grammes were taken (20% of the peak take of rusk) without appreciable effect on the postbait. These failures were therefore not due to unpalatability but to non-toxicity, though the substance had been stated to be as toxic as Antu. Chemical analysis confirmed that the sample was indeed 2-biphenylthiourea.

(*h*) **Conclusions**

This survey has shown that most infestations of brown rats can be successfully controlled after prebaiting on an approved system and with the correct base and concentration of poison. There are many variables which preclude a decisive statement about relative efficiency of the different poisons but the summary in Table 25 (*a*) gives a rough guide. There was little difference between the results with arsenic, zinc phosphide, or squill extract, which were 85–100% successful in the great majority of cases. Red squill powder at 10% in rusk was much less efficient, and neither it nor the extract was used in some of the more difficult environments in which the other poisons were tested. Squill powder needs to be tested at a higher concentration or in a wetter base. Little information is available for Antu, which appears to be highly efficient, or for barium carbonate which should probably be used in certain types of habitat only.

TABLE 25. *The comparative efficiency of prebaiting and direct poisoning*

	(a) Prebaiting (mostly 1–3–5 system)						*(b) Direct poisoning*		
			Red squill						
% kill	*Medium arsenic*	*Zinc phosphide*	*Powder*	*Extract*	*Antu*	*Total tests*	*Zinc phosphide*	*Antu*	*Total tests*
85–100	32	27	7	11	6	83	16	6	22
70–	1	3	7	1	..	12	12	2	14
55–	2	2	2	1	1	8	6	1	7
40–	1	..	..	..	..	1	6	3	9
25–	1	..	..	..	..	1	2	1	3
0–	..	..	..	..	..	..	2	1	3
Total tests	37	32	16	13	7	105	44	14	58
% 'successful'	86	84	44	85	86	79	36	43	38
% killing < 55%	5	0	0	0	0	2	23	36	26

Prebaiting results are taken from Tables 51–53, omitting the following for reasons given in the text: (*a*) fine and coarse grades of arsenic (samples 1, 8, 20, and 7); (*b*) medium arsenic at 10% in sugar-meal; (*c*) zinc phosphide in flour; (*d*) red squill powder of low toxicity (sample S 2 M.I.); (*e*) red squill extract at 5%; (*f*) Antu at 1%. Direct poisoning results are from Tables 5 and 54.

12. DIRECT POISONING

The data about direct poisoning, i.e. without prebaiting, fall into five groups: *miscellaneous poisons*, 12 tests (text and Table 22); *zinc phosphide*, 14 tests in 1943–4 (Table 54 (*a*)), 22 in East Anglia in 1945 (Tables 4, 5), 19 others not checked by census baiting (text and Table 54 (*c*)); and 16 tests with *alpha-naphthylthiourea* (text and Tables 5, 54 (*b*)). These 83 tests present a strong empirical case against the efficiency of direct poisoning.

(*a*) **Miscellaneous poisons**

Five tests were made of a commercial poison bait containing 2% yellow phosphorus, which, following the directions given, was spread on bread and fat. Two ricks were baited in holes in the base and in the ground nearby. Two weeks later, at threshing, one half the population of seventy-four rats was found to have been killed. The other four tests of this bait were done while the populations were still being census baited on wheat (Table 22) and only one was successful (peak takes: (1) 85 g., (2) 0 g.).

Another commercial bait containing 2% zinc phosphide was tested under similar conditions, the approaches to the pile of wheat being covered with the bait. As there was no measurable take of the poison the wheat was removed, but still no poison was taken. Finally, another of the same firm's preparations containing 5% zinc phosphide was spread on bread and distributed in holes and on the approaches to the wheat. All the twenty-one bread cubes were removed by the rats but without significant results (Table 22).

Red squill extract at 10% in rusk was used on a large population on a rubbish dump. Eighty hole baits with 10% extract were given, with negligible results. Another dump population was reduced about 90% with 2% extract and a few rats in a chicken run were all killed by 10% extract. The respective peak takes of wheat in census 1 were: 2,285 g., 940 g., and about 100 g.

Only one properly measured test was carried out with arsenic (sample 31) and this was 100% successful (the peak take of wheat was 385 g.).

(*b*) **Zinc phosphide**

(i) *Measured tests*

Only six out of the first series of fourteen tests were successful (Table 54 (*a*)), better results being achieved with sugar-meal (four successes, and three failures of 73–84%) than with rusk (two successes and five failures of 26–71%).

In a later series (Tables 4 and 5) ten tests were done with rusk and twelve with sugar-meal. Judged from peak takes six tests were successful, four of them after sugar-meal. Poisoning was done between noon and 5.30 p.m. B.S.T. on one of the days between 27 February and 4 March during a period of dry weather. Failures with both baits occurred at random on the different days and irrespective of the time at which baits were laid. There was no suggestion of a correlation with any other factor such as size of population or scarcity of alternative food. Rzóska points out that as extensive feeding may occur in the day-time under conditions of food scarcity there is a chance that certain rats will have fed just prior to the laying of poison baits, thus nullifying any advantage from food scarcity.

Eight tests (Table 54 (*c*)) were measured by dusk counts or total population counts. Of the latter, one was carried out in the P.L.H.A. by Mr. T. G. Edwards: 118 rats were dug out of an earth-bury 2 days after the poisoning, thirty-three of them still alive. The other populations were recovered from ricks or baled straw (Plates 2 & 5). In this series of dusk and population counts half the tests qualify as successful, though only one failed badly.

(ii) *Other evidence*

Nine tests on small populations were roughly checked by counting reopened holes. Two were failures; but the successes and those achieved with some other very small colonies suggest that direct poisoning may work better than it does against large populations (but see § 16 (*c*)). Two other tests were checked by general evidence only: one, on a large rubbish dump, seemed to be successful, the other, covering about 2 miles of walls and hedgerows, left about 20% of the holes and tracks still in use.

Interesting results were obtained at thirteen localities where direct poisoning was carried out with the object of leaving as many survivors as possible for other experiments (§ 17 and Ch. 14). As reduced amounts of bait were given these do not qualify as proper tests of method. Five per cent. zinc phosphide was used in each case with soaked crushed barley, soaked crushed maize, or soaked wheat. Each of the first two baits eliminated one out of four populations whereas soaked wheat killed four out of five.

(*c*) **Alpha-naphthylthiourea**

The high toxicity of Antu made it seem an admirable poison for use without prebaiting. The L.D. 50 of zinc phosphide is 41·3 mg. per kilo and of Antu 4·2 mg. per kilo. One gramme of rusk with 5% zinc phosphide therefore contains 3·6 A.L.D.s per rat of 333 g. body weight; with 2% Antu there are 14·3 such doses per gramme. As two preliminary tests at 2% on soaked wheat gave complete or almost complete kills, Antu was tested on a larger scale in Northumberland.

Nine infestations were census baited and, after a 3-day gap, were hole-poisoned with 2% in rusk. A further test, in sugar-meal, was done without census baiting; but at the same time zinc phosphide was used in an adjacent stretch of hedgerow that had been prebaited on the 1–3–5 system. Holes were then blocked and the two results seemed to be about the same (Table 2). In the meantime four more tests were done in Oxford, making a total of sixteen. One of these latter (peak take 435 g. in a fish shop) was obviously a bad failure, but is omitted from the analysis as the kill cannot be estimated.

Of five tests using soaked wheat or sugar-meal all but one were successful (Tables 52, 54 (*b*)). With rusk, three out of ten gave good results, two were slight failures and five killed less than half the population. Nine of these tests have already been analysed in Table 5 and it was shown that conclusions are much the same whatever use is made of the figures. This table also shows that results were not obviously affected by the food supply.

(*d*) Discussion

In spite of the great toxicity of Antu compared with zinc phosphide the results of direct poisoning were about the same (Table 25 (*b*)). It may therefore be concluded that if it is at all possible to lessen the variability of results with direct poisoning the best approach is through improving the base. Particularly good results have been achieved with both poisons when used with soaked wheat. The evidence from Tables 5 and 54 (*a*) and (*c*) is at least suggestive that sugar-meal is the better base for direct poisoning with zinc phosphide, and the results with Antu are not inconsistent with the same view. This is rather unexpected, for after prebaiting, rats tend to eat rusk twice as fast as they do a dry bait (Table 34), and prebaited rats killed with zinc phosphide in rusk have bigger stomach contents than those killed with sugar-meal as the base (Rzóska, unpublished).

Many practical men agree that satisfactory rat control is possible with any one of several poisons or bases, but the value of prebaiting is challenged both in this country and the United States, where scientific opinion is also against it. Prebaiting was not officially recommended in Great Britain before the recent war. There are obvious practical objections to prebaiting and some that have been raised are as follows: time, labour, and bait are wasted and the organization is more difficult, e.g. in relation to the working week or because of the travel involved or because volunteers, where needed, would lose interest or not have the time (Emlen, 1947). It has also been claimed that rats are attracted from adjacent premises. This objection seems most unsound, for rats which can be drawn in by prebaiting are likely to come in later whatever method of control has been used. A further objection sometimes raised is that prebaiting is more expensive. Again this view is unsound since it takes no account of the value of the results compared with those after direct poisoning.

The literature is almost entirely lacking in sufficient data to reconcile conflicting viewpoints, but Dopmeyer (1936) gives an example of the rejection of direct poisoning in favour of the more expensive but efficient procedure of clearing rat harbourage. This author investigated rat control in the Hawaiian islands and found that even where success was claimed 'no data were available and that results were based only on general observations'. He found that poisoning of *R. rattus* and *R. hawaiiensis* was being done on an immense scale with baits of 10% arsenious oxide, 0·4% thallium sulphate, or barium carbonate. He set aside experimental areas of 500–1,500 acres of sugar-cane or pineapple which were trapped for a week, poisoned, then retrapped. No significant difference was found in the number of rats per 100 traps before and after poisoning with arsenic, even on one 500-acre plot which received 50,619 packages of bait. (Each package was wrapped in a 4 in. square of waxed paper and, see Pemberton (1925), probably weighed ½ to 1 oz.) A plague zone treated with up to 354,098 arsenic baits per month and two towns receiving up to 46,842 thallium baits per month also showed no drop in trapping rates.

Most of these areas had been poisoned 2–10 weeks before these tests, which perhaps demonstrate more the existence of an enormous bait-shy population than the effect of a first treatment by direct poisoning. However, there were two areas, not treated for 5 months, which showed equally poor results.

In other cases it would also be better economy, even at greater cost, to adopt a method of virtual elimination, rather than to continue poor control by cheaper methods. With prebaiting, however, there is no reason to assume that the cost, on an annual basis, is higher than that of direct poisoning; for the latter will generally involve more frequent treatments. In our own experience results after prebaiting have been so markedly superior to those after direct poisoning that the cost of treatment is the least relevant feature.

In two early papers, which are exceptional for the amount of field evidence presented, Boulenger (1920) counts as successful those of his fifteen field trials in which any mortality was proved, particularly by the presence of corpses; and Pemberton (1925) quotes general approval of results where the best index of success was a decrease in trapping rate of slightly over 50%.

Emlen and Stokes (1947) assessed the results of direct poisoning by municipal workers in Baltimore. Percentage reduction in numbers was estimated for groups of five city blocks treated with various baits, of which those with Antu were the most successful. In the following extracts from their data (Table 26), figures for the indoor rats have been worked out separately, for although it is stated elsewhere that the indoor surveys were less accurate (Emlen, Stokes, and Winsor, 1948) there is no reason to believe that the mean is biased, and the results may reflect the difference in habitat.

TABLE 26. *Results in Baltimore*

Base	*Habitat*	*Est. no. rats*		*% reduction with Antu*	*% Range (5 blocks)*
		Before	*After*		
Fine ground maize	Backyards	351	87	75	41–88
	Indoors	234	99	58	. .
	Whole block	585	186	68	45–81
Several (p. 242)	Backyards	224	15	93	62–100
	Indoors	151	54	64	. .
	Whole block	375	69	82	62–92

Since these operations were not carried out by a research staff they cannot be compared with the results of our own tests of prebaiting (which gave better results in a wider range of conditions); but the Baltimore tests were nevertheless more successful than direct poisoning by our own staff.

In practice the skill of the operator must be a deciding factor so that incompetent jobs such as those given in Table 41 might better have been done by a cheaper method. On the other hand, with the skilled men whose work is described in Chapters 10 and 13, prebaiting methods can maintain effective rat control. This is often likely to be more practicable (though less desirable on other grounds) than improving the environment, the alternative discussed by Davis (1948) for cases in which direct poisoning breaks down in practice.

The U.S. Fish and Wildlife Service has probably the greatest single influence on rodent control in the United States; but comparatively little of the field experience of the workers in Control Methods Research is available for review. A visit in 1946 satisfied me that their belief in the efficiency of direct poisoning, particularly with '1080', was based on a number of spectacular clearances; but the comparative efficiency of this and other poisons cannot be assessed from the information so far published (Kalmbach, 1945, 1947, 1948; Ward, 1946). Emlen and Stokes (1947) and Barnett and Spencer (1949) report rather poor results with '1080'.

In the official pamphlet of the U.S. Fish and Wildlife Service, Silver and Garlough (1941) advise prebaiting before using red squill powder, and 'in stubborn cases, or when one is willing to go to additional trouble to increase the chances of success'. Decker, Gunderson, and Barre (1941) in Iowa, recommend prebaiting only before second and third treatments with squill or barium carbonate, first treatments being stated to give kills respectively of 60–90% and 80–100%.

Raebiger (1931) reports that the use of Ratin cultures in 1,022 towns and villages cleared 98·65% of the infested places; but even where less incredible kills are claimed, the small improvement apparently left for prebaiting methods may not seem worthwhile. Nevertheless, as argued in § 8 (*l*) there may be a great difference between the long-term efficiencies of treatments which give kills above rather than below 90%. It therefore seems profitable to use methods which most consistently offer this degree of control.

There is no doubt that complete kills are often achieved with direct poisoning, but results are far less consistent than if prebaiting is used. Other series of tests with improved bases or toxicities might show trends towards consistently higher kills, but this was a long-term and more expensive form of research than was justified in view of the established success of prebaiting. Table 25 summarizes our information about both systems and shows that twice as many of our tests were successful after prebaiting (85–100% reductions) as after direct poisoning. Furthermore it was exceptional, even under difficult environmental conditions, to kill less than half the rats after prebaiting, whereas direct poisoning frequently failed to kill this many.

The various factors discussed in previous sections make it impossible to give a figure for the relative efficiency of the two baiting systems. For this a series of properly paired trials and comparable baits would obviously be necessary; but inasmuch as the tests were carried out in an unbiased manner they provide reasonable evidence that direct poisoning is not a reliable way of using quick-acting poisons against the brown rat.

13. PROBLEMS OF POISON BAITING

Empirical results show that under many circumstances rats do not eat a lethal dose. The onset of illness then makes them unable or unwilling to take any more poison bait (Ch. 8) and this is the central difficulty which governs all attempts to control rats by poisoning.

(*a*) **Factors limiting success**

If rats willingly consumed a second or third dose of the same poison bait and a reasonable average kill were obtained each time, a population could be reduced in comparatively few treatments without prebaiting. However, since only the first use of a poison bait is generally successful, new ones would have to be used each time. With a sufficient range of bases and poisons this would no doubt be possible, the only disadvantage over prebaiting being the more frequent risk of poisoning other creatures and the greater waste of poison. If a restricted supply of different materials make this course impracticable other techniques must be tried. A poison or base which did not produce shyness would be of considerable value in rodent control (§ 19). A bait of red squill extract which white rats will readily accept on later occasions (Ch. 2) is refused by brown rats (§ 17 (*b*)).

The majority of inorganic poisons act swiftly and little research has been directed towards slowing down their action or finding ones that take several hours to act. The most valuable property a poison could have would be slowness of action. Bacterial preparations have the value that they take several days to affect the rats; but they are otherwise objectionable (Leslie, 1942) and relatively inefficient (Doty, 1945). Few inorganic poisons so far employed have a sufficiently delayed action at normal concentrations to enable a rat to continue feeding even 1 hour after a sublethal dose of any size. Thallium sulphate is probably the most desirable from this point of view, and since arsenic acts more slowly if coarsely ground, this advantage may more than offset its lower toxicity. Symptoms of illness are delayed if the concentration of poison is reduced, and intake of bait increases in inverse proportion (Ch. 8, Appendix 2). At a certain point, however, the intake of poison starts to decrease so that the most efficient balance between speed of action and poison intake is a delicate one. At present the best concentration for field use must be determined empirically (Ch. 7). Something could no doubt be done to delay the action of the ordinary poisons but it is doubtful whether a much greater intake would result.

One of the remaining lines of attack is therefore to increase the toxicity of the bait so that even the smallest mouthful contains a lethal dose; but with most poisons this can be done only at the risk of making the bait unpalatable. Research in the U.S.A. has provided two powerful new poisons, but our own conclusions are that the discovery of increasingly toxic poisons may not be altogether profitable. Resistance to a poison is one of the biological variables and the minimum intake of poison has to be enough for the largest and most resistant rats. Even in those killed after prebaiting there is a considerable range of stomach contents, both within a population and in the averages for different ones (Ch. 9). Other rats survive altogether. With direct poisoning there is the added variable of the reaction to a new food. It is therefore doubtful whether higher kills can be expected to follow increases in the toxicity of the bait beyond a certain point.

Other difficulties arise when the toxicity of a bait is to be raised so that a L.D. 100 (with some poisons a purely theoretical figure) is contained in the smallest expected intake: the mixing may not be done with the requisite thoroughness, and such baits, unless rodent-specific, are too dangerous to man and other animals. Further, the method of laying poison baits is of considerable importance: far more so, it is to be supposed, with direct poisoning than with prebaiting, which to some extent overcomes deficiencies in the operators' technique by making the rats seek the baits.

Finally, successful control can be achieved if the speed of feeding is sufficiently great. Under many circumstances this happens without special measures as shown by the 100% kills with simple baits given direct; but these successes cannot be predicted as the factors are unknown. In the long run it might be profitable to treat all infestations by direct poisoning, with the expectation that in some cases no further work need be done; but with a limited choice of materials for retrieving a failure this course is undesirable. Steps must therefore be taken to bring about the maximum feeding rate either by using a highly attractive bait or by prebaiting.

Since rat populations differ considerably in their food preferences it seems unlikely that any one bait can be made so attractive that all rats will eat it rapidly. Furthermore it is very doubtful if any rat with its stomach well filled with normal food is capable of a high rate of ingestion, and this previous feeding is likely to occur more often if the rats have not been prebaited. Thus there is probably a limit to the efficiency to be expected from making direct poison baits more attractive, though it has not necessarily been approached so far. Silver and Garlough (1941) state that 'experience has taught that the kind of poison used is generally of less importance than the bait', but emphasis in the United States has since shifted towards a search for better poisons.

There are two practical objections to the use of the more elaborate baits. They are generally less readily available than a few simple standard foods and may require a good deal of processing. Individually wrapped baits, strongly advised, for instance, by Decker, Gunderson, and Barre (1941), may, according to Eskey (1934), take as long to wrap as to distribute.

Sandwich baits or those made up with bananas (Anon., 1939) or grasshoppers (Jack, 1938) are also tedious additions to large-scale control. Secondly, in the present state of the world's food supply the more attractive-sounding mixtures such as those of Hogarth (1929), Schlupp (1920), or Holsendorf (1937) which include cheese, honey, or eggs are unlikely to be used nowadays. The bait bases recommended for Antu (Richter and Emlen, 1946) are, with the exception of two of their items, also impractical in many parts of the world; they include 'dried apples, sweet potatoes, tomatoes, cantaloupes, watermelons, ground meat, the white and yolk of eggs, and fish or chicken heads'.

Poisons cannot generally be used in such a way that their action is independent of the feeding or drinking behaviour of the rat, though sodium silicofluoride and Antu may be spread on runways so that the rats get poison on their feet and fur and then lick it off. A British commercial preparation can be used in a similar way, smeared round holes, but neither method has more than limited possibilities in Great Britain. Spraying water supplies has also a restricted value.

(*b*) **Effects of prebaiting**

Probably one of the chief advantages of prebaiting is in overcoming the rats' suspicion of an unfamiliar food supply. This problem is discussed in Chapter 5 and is only briefly illustrated here in Fig. 10 which shows the rats' sensitivity to a comparatively minor change in the environment and their slow adaptation to a new food, flour, which was later preferred to the original wheat. It is also interesting that the removal of the flour caused a temporary drop in total cereal take.

For securing a large intake of poison bait it should be an advantage to have the rats encounter it before they have fed elsewhere. Stomach analysis, in fact, showed a general correlation of high poison bait intake and the absence of food already in the stomach (Ch. 9). It is also known that rats, hungry from underfeeding on previous days, eat more rapidly and stay for a longer time during their first hour than they do after good feeding on previous nights (Chitty and Shorten, 1946). It is also found that after this first hour the rate of feeding is somewhat less (Table 30 (*a*)).

It might be supposed that as a rule well-fed rats would also eat more rapidly at their first meal each night. However, this is not always so, and though the total rate may tend to be higher at the start more rats are probably feeding simultaneously. Table 30 (*b*) shows that well-fed rats did not necessarily feed any faster or longer on their first appearance than subsequently: most fed equally well or better when they reappeared later in the night. Similar observations were made on two other occasions during surplus baiting (Chitty and Shorten, 1946). However, while the speed and duration of feeding are not always greater in the early part of the night, there seems to be some such tendency when the food is familiar. Through prebaiting rats learn to visit baits in containers before foraging elsewhere, though sometimes the process may be slow (§ 14, Fig. 12). However, by hole baiting it is generally as easy to intercept rats by direct poisoning as by prebaiting, so that other factors are probably more important.

Prebaiting, by adding to the food supply, should, if anything, make the rats less hungry. Certainly the prebait should not be so plentiful that if the rats are normally hungry they cease to be so by the time of poisoning. Therefore token prebaiting is often to be preferred to a surplus, and presumably a gap before poisoning also has the advantage of ensuring that the rats are not overfed. The amount provided in practice, however, almost certainly bears a highly variable relation to population size.

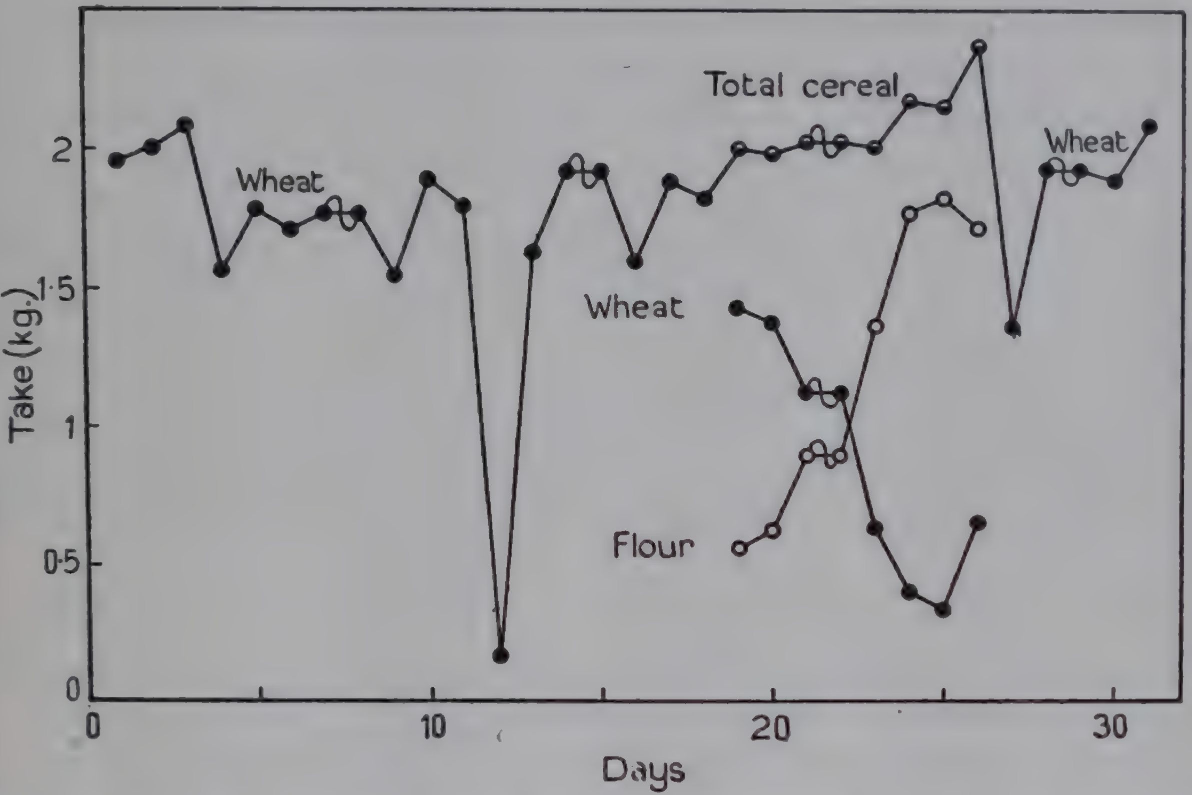

FIG. 10. 'New object reaction' among rats at the experimental colony. From the twelfth day wheat was placed in two old tin trays instead of in two piles on the floor. During the choice between wheat and flour the contents of the two pans were switched on alternate visits. Wavy lines join averaged takes.

It has already been shown that rats with an abundant natural food supply may take relatively large amounts of surplus bait during the first day or two (§ 6 (*b*)). This initial eagerness for the bait must be because it has some attraction for them which wears off when they have eaten it often enough. Rats in ricks or on well-provisioned farms will take the second and later token baits soon after they are laid and it seems unlikely that this same daytime response would be maintained if the bait were available in unlimited quantities. On the hypothesis that the comparative novelty must be maintained, baiting with gaps may be a particularly suitable system for this reason as well, though experimental evidence is at present conflicting. It might be thought that rats which take large amounts of bait during the first night (Table 8 (*c*)) might be more readily controlled by direct poisoning than prebaiting. This problem should receive close study, for in such environments prebaiting may give most disappointing results (§ 16 (*d*)). However, the rate of intake of bait during the first half hour is not necessarily as high as it is during the rest of the night.

Control by the 1–3–5 system of prebaiting in holes has already been shown to be highly efficient. Good results have also been obtained by prebaiting without gaps or with only one, but there have also been a few failures which may or may not have been due to the system. Only limited studies have been made of variations in prebaiting method.

In corn-ricks, poisoning was carried out after prebaiting once or twice: both with and without a gap between operations (Ch. 10). Where one prebait was given on the day before poisoning, less than half the rats were killed with arsenic compared with 89% on the 1–3–5 system. One prebait followed by a gap or two prebaits without gaps killed 81–82%. With zinc phosphide the three different systems gave equally satisfactory kills. In six other habitats variable results were achieved when poisoning was done after one prebait and no gap (Table 27). Three of the six tests were failures, all with zinc phosphide; the only one with arsenic was successful.

TABLE 27. *Field trials of poisoning in rusk after one prebait the previous day only*

No. of holes baited	*Bait used (kg.)*	*Poison*		*Peak take of wheat (g.) in census 1*	*% reduction*
		%	*Kind**		
18	0·4	5	z	370	100
† 123	2·4	5	z	2,080^{+}	90
104	2·0	10	a	1,750	89
34	0·8	5	z	420	0
111	3·5	5	z	700	35
84	3·0	5	z	850	61
117	2·6	5	z	490	89

* a = arsenic; z = zinc phosphide.
† Paired tests; census 2 lasted 10 days.

Any explanation of these failures can only be speculative; but a large amount of prebait was used relative to the estimated population. In all our trials prebaiting was carried out at all available sites as it would be by the practical operator and such amounts of bait were typical. It may be that these quantities contributed to the failures. The rats may have altered their feeding rhythm unfavourably or the prebait may not all have been eaten or made inedible within 24 hours, and so have 'diluted' the poison bait. These tests show that the 1–3–5 system has advantages over some others, though the reasons are not yet clear.

In order to study this point Thompson mapped and labelled all the rat holes on a rubbish dump and compared the effects of continuous and gap baiting. About 20 g. rusk was laid at 11 a.m. just within the holes and records were made each hour until 4 or 5 p.m. of the number partly or entirely taken. Five days continuous prebaiting was carried out at seventy-nine holes and by the fifth day about twice as much bait had disappeared in a given time as on the first day (Fig. 11 (*a*)).

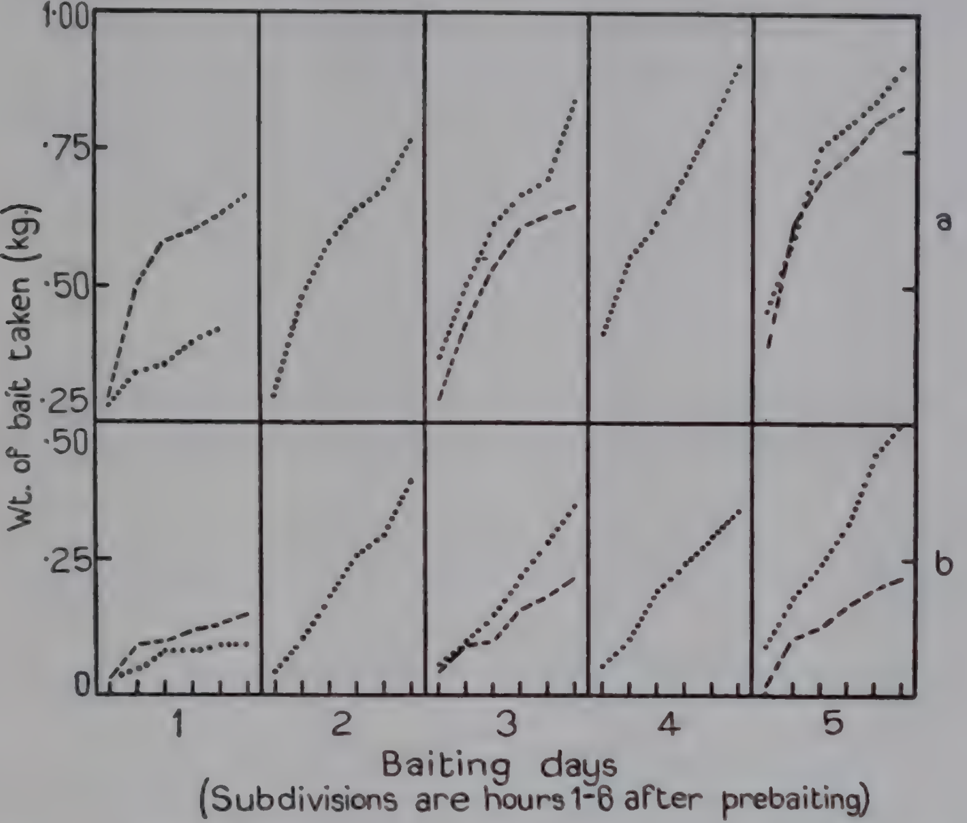

FIG. 11. Rate of disappearance of sugar-meal from rat holes during daylight baiting on days 1, 3, and 5 (broken line) and continuous baiting for 5 days (dotted line). Two rubbish dumps (*a*) and (*b*). The take of bait was estimated by eye, but similar trends are obtained by plotting the number of baits completely cleared.

Ten days later these same holes were baited on the 1–3–5 system. The rats were almost certainly affected by their previous baitings, as they took the bait faster than they had on the first day before. Despite this initial advantage, however, the speed of feeding on the third and fifth days was not as great as it had been without the gaps. If the second and third baitings are compared on the two occasions there is little difference according to the system.

After about 3 weeks the same procedure was started at 5 p.m. (sunset) with gap baiting. Feeding rates were higher than they had been in the day-time but did not increase on successive days. With continuous baiting (after an interval of 10 days) a rather higher initial feeding rate was reached by the third day but not exceeded later: 1 kg. of bait had gone by 9 p.m.

On another dump day-time observations were made at forty-seven holes (Fig. 11 (*b*)), starting with gap baiting. There was little progressive increase in speed of feeding and no evidence 10 days later that the rats were still conditioned. Continuous baiting started with slow feeding, improved on the second day, and then changed little before the fifth when at least 2–3 times as much bait was taken as in corresponding periods at the start.

These experiments include several points of interest. Rats were obviously active in their holes in the day-time as evidenced by the first days' takes; but they did not feed as freely as during darkness. Prebaiting increased the rate of disappearance of the baits, though this may have been because more rats attacked them and not necessarily because individuals ate faster. Five days continuous baiting was on the whole superior to 3 days, whether continuous or with gaps. There was little difference between a 1–2–3 and a 1–3–5 system, certainly no proof that gaps increased the speed of feeding.

(*c*) Summary

1. Intake of poison bait is limited to a short period after the first mouthful (Ch. 8), and unless the bait succeeds on this occasion it is generally of no further use (§ 17; Ch. 8, Appendix 1).

2. There are certain practical difficulties about using baits which are highly toxic or require elaborate ingredients and it is not certain that they will always work.

3. During prebaiting rats overcome their suspicions and learn to seek the bait before feeding elsewhere (Chs. 5, 9).

4. The merits of different systems of prebaiting are not yet understood; individual feeding rates probably increase on the first few occasions; prebaiting with gaps may be an insurance against surfeiting the rats but may not always be necessary.

14. PREBAITING IN HOLES AND CONTAINERS

(*a*) Advantages of hole baiting

When containers are used rats are no longer forced to encounter the prebait before leaving their holes, and may take longer to change their feeding habits. There are, of course, many infestations in which containers may be placed between the rats' living and feeding quarters; but if the bait is not much more attractive or easily obtained than the natural food there may be little incentive to abandon normal feeding habits.

The greater difficulty of prebaiting rats in containers than in holes was illustrated at the experimental rat colony by Miss Shorten and myself. The rats had received only occasional baits for about 2 weeks and were then given a surplus of wheat in the food-recording machine. Some rather higher initial takes were no doubt because the rats had been underfed. After 6 days, hole baits were given at seventy-eight points to observe their effect on the rate of feeding on wheat. 1,890 g. rusk was placed at 2.15 p.m. B.S.T.

and this procedure was repeated on the third and fifth days. No trace of any prebaits remained the following mornings.

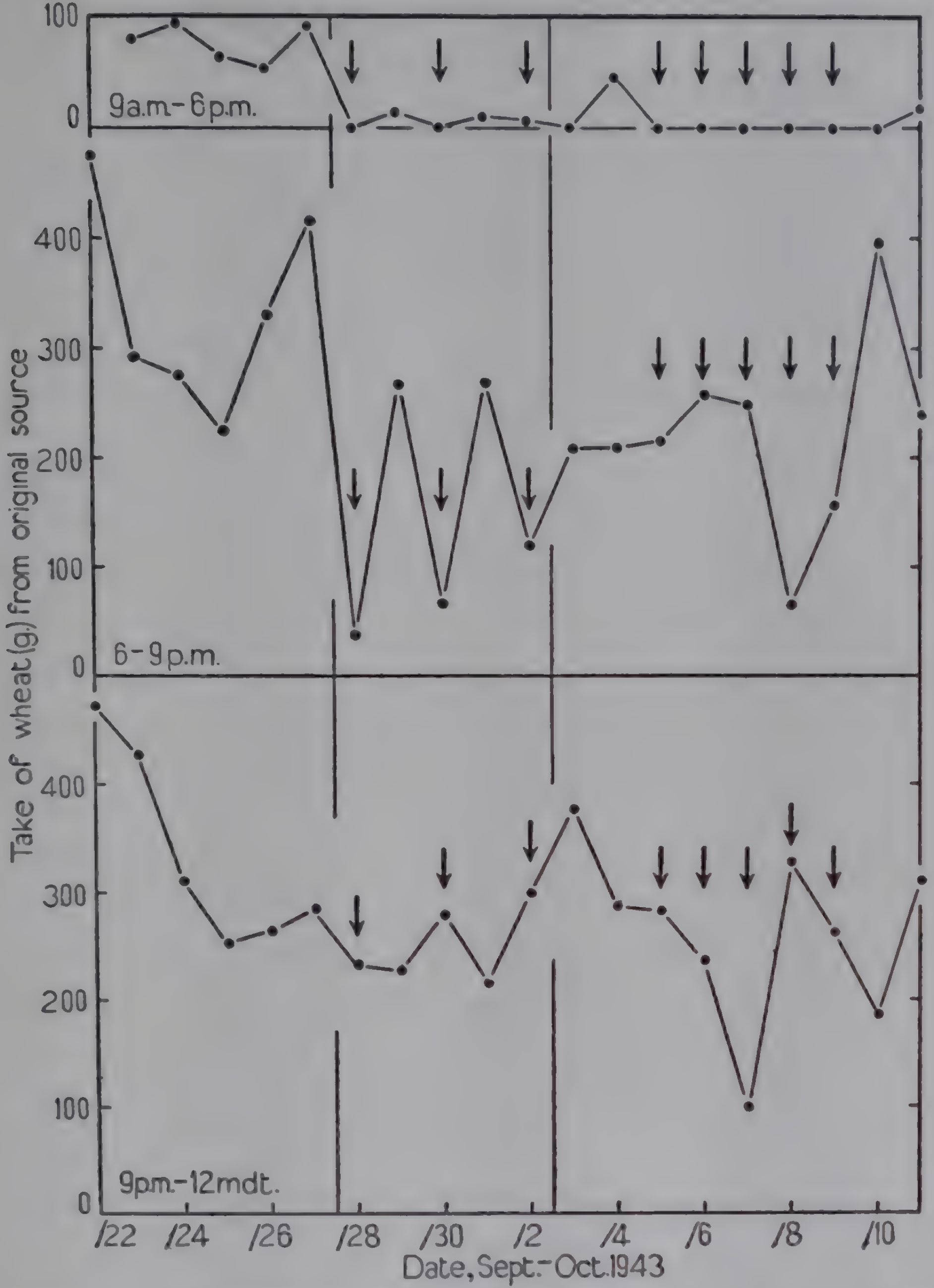

FIG. 12. Comparative efficiency of prebaiting in holes and containers. Graphs show the amount of wheat taken from an accustomed source up to midnight. Arrows in central upright panel show days of hole baiting with 1 : 1 rusk, those in right panel show days of baiting with 1 : 1 rusk in containers (Table 28).

On the first day of hole baiting no wheat at all was taken until 8 p.m. whereas previously a certain amount had gone during the afternoon and considerably more from 6 to 8 p.m. Fig. 12 shows how effectively the rats were prebaited during the 3 hours 6–9 p.m. and how normal wheat feeding was resumed at this time on the gap days. Feeding during the afternoon, however, had ceased almost entirely. On the fifth day less rusk was provided and, perhaps as a consequence, there was rather more wheat feeding before 9 p.m.

Two days later 360 g. rusk was supplied in each of five P3s which had been placed more than 2 weeks before. The same amount was given fresh on the third and fifth days; but none of the baits was finished in 24 hours and some was left after 48 hours. In effect, therefore, prebaiting was continuous for 5 days. Residues were weighed daily, except on the first occasion, and finally removed after 5 days.

At the start of container baiting the normal feeding habits, except perhaps for the absence of day-time feeding, were not affected before midnight, after which less wheat was taken than usual. On the third day wheat feeding was normal up to 10 p.m., then practically ceased until midnight; but on the next 2 days the containers were evidently visited from 6 p.m. onwards. This slow adaptation is in marked contrast to the way in which hole baits intercepted the rats from the first.

In this comparison between hole and container baiting, rusk was used to prebait rats accustomed to wheat, which was probably the preferred food. Because of their position the hole baits were nevertheless taken at once. Better results might have been achieved with the containers if there had been more of them, or a better distribution, or if wheat had been the bait and rusk the familiar diet. Such difficulties, however, are met with in practice. The discrepancy between total take of cereal (dry content) and that of wheat alone (Table 28) is consistent with our experience in the outdoor cages that much of the hole baits is wasted; but it is also possible that rusk and wheat together may have competed slightly more efficiently with the natural food than did wheat alone.

TABLE 28. *Total takes of bait (g.) during the experiment in Fig. 12*

Date	*Take of wheat from hopper*	*Take of rusk (cereal content)*	*Total wt. of cereal disappeared*
/22 to /27 Sept. . .	1,260 to 1,580	. .	1,412 (mean)
/28 Sept. . . .	860	945	**1,805**
/29 ,, . . .	1,270	. .	1,270
/30 ,, . . .	920	900	**1,820**
/1 Oct. . . .	1,320	. .	1,320
/2 ,, . . .	1,145	700	**1,845**
/3, /4 Oct.. . .	1,050, 1,570	. .	1,310 (mean)
/5 to /9 Oct. . .	1,035 to 1,410	240 to 455	**1,540** ,,
/10, /11 Oct. . .	1,450, 1,530	. .	1,490 ,,

(*b*) Empirical results with containers

It has already been explained that when both census and poisoning are carried out at the same site the results are suspect as a measure of the extent to which an area has been cleared of rats. No quantitative evidence can therefore be given to support the statement that we were able to reduce small infestations satisfactorily by poisoning in pipes or P3s. We can show, however, that sites which we had hoped to clear by this method were not always freed of rats. As shown in Fig. 3 (*b*) the per cent. reduction in take of census bait may not measure the extent to which an area has been cleared; nevertheless a high postbait is sure evidence that rats were left despite the attempt to clear them all out.

Many tests were carried out by first census baiting the population with wheat in containers, then immediately giving surplus or token baits of another base, followed 3 or more days later by poison. In a practical control operations no census baiting would be done, so that such tests, during which the rats are drawn in, are no proof that token baiting alone would have sufficed to prebait them. Despite this advantage the tests in Table 29 were not obviously successful. The list is rather small because it excludes tests in which less than 500 g. bait was taken in census 1 or in which bait formulae were used other than the standard ones discussed in § 11.

TABLE 29. *Field tests of control by poisoning in containers: relationship between peak takes of census bait*

Habitat	*No. of containers*	*Poison*		*Base*	*Peak take of wheat (g.)*		*Duration of census 2 (days)*
		%	*Kind*		*Census 1*	*Census 2*	
Farm	1 P3	10	e	R	1,000	130*	6
Dump	10 ,,	10	a	R	1,960	630	5
Pig farm	4 ,,	15	a	S	1,015	200	7
Allotment	2 ,,	10	e	R	530	0*	8
Butcher's shop	4 ,,	10	e	R	505	185	13
Dump	12 pipes	5	z	R	1,015	700	10

e = squill extract; a = arsenic; z = zinc phosphide; R = rusk; S = sugar-meal.
* Apparently successful.

In the first two tests in the table token baiting was carried out before poisoning; all other populations were surplus prebaited throughout. The peak in the second census is not confined to takes during the first 5 days since in the present instance we are concerned with the number of survivors left on the whole area and not with the comparative efficiency of the bait (§ 4 (*b*)).

It cannot be claimed that these tests are a fair sample of the value of containers in rodent control; they should certainly not be condemned until a more up-to-date attack can be made on the problem. There is a great need for containers in many environments where accidents might occur through contamination of food. Poisoning at numerous points in food shops (Fig. 16, for example) obviously cannot be recommended for all grades of operator, whereas container baiting is relatively safe.

(*c*) **Summary**

1. Prebaiting in holes is more efficient at intercepting the rats before they reach their food supply than is prebaiting in containers.

2. Container baiting requires more time to produce a change in feeding behaviour and probably demands a relatively more attractive bait.

3. Practical results (not quoted) with containers suggest that they are excellent for use against small populations, but that larger ones are not always controlled.

15. BEHAVIOUR TOWARDS BAIT BASES

The war restricted the choice of foods for rat baits and there was less need to study the base as intensively as the poison. However, a good many of our observations on the feeding behaviour of the brown rat are fundamental to an understanding of the problem of choosing a suitable base.

(*a*) **Feeding behaviour of a marked population**

The following experiment was carried out at the experimental colony by Miss Shorten and myself. The rats were very hungry; but apart from a big appetite they showed no unusual features. The normality of their behaviour on this and other occasions was later confirmed by watching a population feeding in complete darkness, using an infra-red apparatus (Southern, Watson, and Chitty, 1946). Notes on each rat are given in Appendix 2 and preliminary details have already been published (Chitty and Shorten, 1946). Thompson (1948) and Barnet and Spencer (1951) have carried out similar studies.

(i) *Methods*

Fifteen rats were trapped on 25 January 1944 from a population that had been token baited at noon for over a month. All but one of the rats were marked and released next day and artificial lighting was put on at night. When watching started a week later only eleven animals were identified; the missing animals were 150 g. ♀, 170 g. ♂, and 190 ♂. Records were obtained of token and surplus feeding behaviour on wheat, then 1 : 1 rusk was given for 2 nights, followed by zinc phosphide. Rzóska examined the stomach contents of seven animals. After postbait takes had been watched the surviving rats were trapped. Observations were lumped in 5-minute periods, which is satisfactory except when poison is being eaten, when the possible error of almost 10 minutes is large in comparison with the time intake continues.

When feeding on wheat, rats mostly carry it away in mouthfuls, a count of which is assumed to give a fairly reliable index of changes in the rate of feeding of that rat. Certain animals sit down and eat from the wheat instead of taking it elsewhere. The only way of recording their activities is to count the number of trips to the wheat and to use a symbol to indicate 'settled feeding'. This does not necessarily introduce a large error (except for non-portable baits such as flour) and in this particular population no rats ate for long without moving away. A fresh visit is recorded for each rat whose settled feeding continues from one period to the next. Another error is introduced by the 'scavengers' which feed on grains dropped by other rats without themselves making visits to the main source. This error was small as only rat No. 9 was involved to any great extent and only on certain days at the start.

(ii) *Results*

Individual behaviour. No. 2 often failed to appear in the first part of the evening and fed only 9 times during the whole night of 9/10 February. It fed remarkably well on rusk. No. 9 at first refused to feed from the tray (which was used throughout) and obtained all its food by picking up what other rats dropped or by stealing from them. Perhaps it had a strong aversion to moving out from the wall or was not then used to the lights; but certainly under the prevailing conditions it took many days to become fully prebaited.

Size of mouthful. The average weight of wheat removed at each visit can be calculated for 8 and 9/10 February when the visits of unmarked rats were also recorded: the averages were 0·405 and 0·455 g. During the postbait only one or two rats were feeding and the average was roughly 0·3–0·5 g. per mouthful.

With rusk the rats probably took about the same weight per mouthful as with wheat whereas on other occasions they took more (Table 33). The average mouthful of poisoned rusk was 0·43 g. wet weight, and three marked corpses yielded the following data: No. 9 (300 g. ♀): 10 g. taken in twenty-one mouthfuls, i.e. 0·5 g. per mouthful; No. 4 (410 g. ♀): 2·1 g. taken in three visits, two of which were settled feeds, i.e. <0·7 g. per mouthful; No. 5 (340 g. ♀) had taken six mouthfuls but only a trace of bait was recovered and this was mixed with other food. Three unmarked corpses contained 6·4, 9·5, and 11·8 g. bait, a fourth had greens and other foods but no detectable poison.

Token baited behaviour. As previously suspected (§ 4 (*a*)), hungry rats began feeding at a faster rate than they did when well fed. During token baiting both intensity and duration of feeding were greater than after surplus baiting. ('Token baited behaviour' continues, of course, for the first hours of the first surplus baiting.) In the hour following its first mouthful each token baited rat, on the average, fed during nine of the 5-minute periods and took an average of 5·4 mouthfuls per period. When surplus baited the rats fed for about five periods in their first hour at an average rate of 3·4 mouthfuls per period (Chitty and Shorten, 1946). Hungry rats also arrived more promptly after the bait was laid.

Table 30 includes data only for two of the days on which the averages above are based. These were the only occasions when sufficient watching was done to bring out the further comparison between initial feeding rates and those during later meals. It is seen that there was a marked drop in feeding rate when the previously underfed rats had satisfied their first

TABLE 30. *Frequency of visits to wheat (per 5-min. feeding period)*

	Period of observation: 7.25 to 10.45 p.m., 5 Feb.						*7.10 p.m. to 8.00 a.m., 9/10 Feb.*					
	(a) Rats previously underfed						*(b) Rats previously well fed*					
	*1st hour**			*Subsequent interval*			*1st hour**			*Rest of night*		
Rat No.	*Visits*	*Periods*	*Frequency*	*Visits*	*Periods*	*Frequency*	*Visits*	*Periods*	*Frequency*	*Visits*	*Periods*	*Frequency*
1	17	5	3.4	37	11	3.4	10	4	2.5	37	21	1.8
0	59	10	5.9	103	13	7·9	11	5	2.2	57	18	3.2
7	64	12	5.3	34	12	2.8	1	1	1.0	127	51	2.5
6	66	10	6.6	39	10	3.9	41	10	4.1	81	20	4.0
3	69	12	5.8	41	13	3.2	44	9	4.9	120	21	5.7
8	39	12	3.2	37	13	2.8	1	1	1.0	65	32	2.0
5	101	8	12.6	46	7	6.6	49	7	7.0	133	32	4.2
10	34	8	4.2	8	3	2.7	33	7	4.7	73	21	3.5
4	28	9	3.1	29	13	2.2	36	8	4.5	102	30	3.4
Total	477	86	50·1	374	95	35·5	226	52	31·9	795	246	30·3
Average	5.6		5.6	3.9		3.9	4.3		3.4	3.2		3.4
2	4	3	1.3	..	..	..	6	4	1.5	3	2	1.5
9	..	..	..	..	..	..	12	5	2.4	91	40	2.3

* Reckoned from first mouthful of each individual.

hunger. Many of the well-fed rats, however, ate no faster at the beginning of the evening than later on. Rats No. 2 and 9 have been excluded from the averages since they did not appear on all occasions. These averages are calculated both by giving equal weight to each individual's frequency of visiting and by pooling the total number of visits.

Feeding rhythm. With token baiting the rats started more or less together, and on 3 February there was a definite slowing down after about an hour. By then the bait had to be renewed and the interruption put an end to feeding for at least the next ¼ hour. On 5 February the initial burst of feeding was spent within 1 hour and practically no feeding occurred again for 1 hour 10 minutes. Then another feeding period started and was continuing strongly 1 hour 10 minutes later still when observations ended.

On the second night of surplus feeding (6 February) there was again an initial feeding period, this time of less than an hour and with far less feeding. After an hour's break, during which it rained, feeding started again, but was interrupted when more rain fell.

On 7 and 8 February individuals were feeding almost at random, though there were breaks when nothing happened at all. During an all-night session on 9/10 February there were decided peaks of activity (Fig. 13) but these tended to coincide with breaks in the wind and rain. Thompson had previously found that activity was much depressed one wet windy day, and the present experiment therefore cannot be regarded as evidence for or against the existence of an inherent feeding rhythm in wild brown rats.

Individual takes of bait. The eleven marked rats appeared to be in the majority and to have about the same weight distribution as the others. Visits of unmarked rats were recorded on 8 and 9/10 February and on the poison night, and each ratio is consistent in showing that the marked rats formed 50–60% of the population (Table 31 and Appendix 2). The ratio of corpses found was three marked to four unmarked which is as close an agreement as can be expected. Roughly then, 1 kg. wheat was eaten by

TABLE 31. *Proportion of visits by marked rats*

Date (Feb.)	*Time*	*Total visits*	*% visits by marked rats*	*Date (Feb.)*	*Time*	*Total visits*	*% visits by marked rats*
8	7.03–8.15 p.m.	230	65·7	10	12–1 a.m.	47	57·4
	8.15–9.30	239	55·2		1–	115	61·7
					2–	134	46·3
					3–	342	54·1
					4–	229	60·7
9	7– p.m.	223	59·2		5–	162	64·8
	8–	194	53·6		6–	286	28·7
	9–	4	58·5		7–8 a.m.	118	67·8
	10–	102		9/10	Total	2,109	53·7
	11–	153	56·2	8, 9/10	,,	2,578	54·9

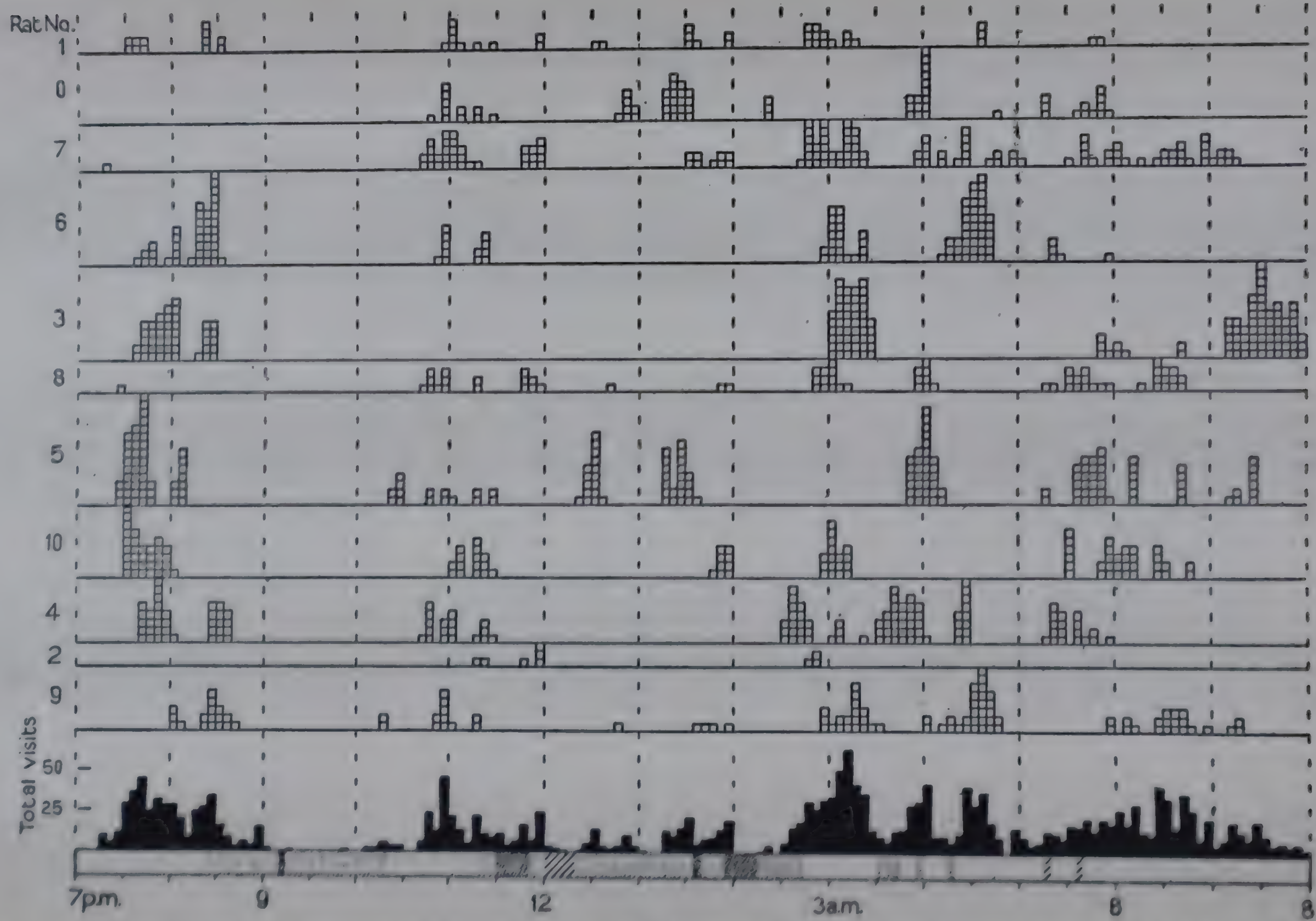

FIG. 13. Distribution and frequency of feeding visits, 9/10 Feb. 1944. Each small cell represents one visit in the 5-minute period. Black columns are total visits by marked and unmarked rats. Bottom row shows weather conditions: light and heavy

twenty rats. Table 32 gives individual figures, and for comparison the upper range reckoned from caged rats (Ch. 6 and Appendix 2, where corrections to body weights are discussed). Neglecting No. 2 which was obviously not prebaited, it is seen that only two or three of the other ten rats ate about what was to be expected from the cage results. The others ate from 33 to 140% more than the expected upper limit. The average take by the ten rats was about 51 g. compared with an expected limit of 34 g.—about 50% more.

Four nights later, however, the wheat take had dropped by about one quarter and, when rusk was offered, remained at about this level in terms of cereal content. This drop was undoubtedly because the rats had been half-starved before experiments started (§ 7 (*d*)). Takes (g.) from /6 to /16 February were as follows (9/10 February in heavy type).

Wheat: 1,000+ 1,160 1,010 940 **960** 1,090 930 860 670
Rusk: 1,370 1,430 (wet take)

The rats had last had rusk on 2 January so that it may have been a familiar food to some of them on 14/ February. Although they did not feed more rapidly on rusk than on wheat there was little evidence of any suspicion towards the changed base.

Poisoning. Poisoning was carried out after only two days' change to rusk because some of the markings were becoming indistinct. Lack of full conditioning and the fact that the rats were not token baited may explain the low take of 60 g. of 5% zinc phosphide. All rats fed for less than 15 minutes with the exception of No. 4 which took a final mouthful between 15 and 20 minutes after the first. The refusal times are quicker than those found in the laboratory, but the takes of poison were nearly all greater than 320 mg. per kilo, the highest dose tested (Ch. 8).

Some rats ate the poison bait less rapidly than they had the prebait; but others ate it more rapidly and the poison did not appear to be distasteful. No. 5 jumped away after first smelling it, but quickly got over its suspicion. No. 1 sniffed the poison and withdrew for 25 minutes but then came back and scavenged, picking up enough to kill itself.

Rats were not deterred by the fact that the preceding animals had stopped eating. The marked rats had finished by 7·25 p.m. except for Nos. 3 and 8 which did not begin until 7·30 and 8·10 respectively; discrimination between poison and old plain residues was shown by No. 7 an hour after it had taken a single mouthful (56 mg. per kilo). One unmarked male was dying within 2 hours of taking the poison. Nos. 4 and 9 were dead within 3½ hours. No. 5 and three more unmarked corpses were found next day.

Postbait. Two rats survived the poisoning (Nos. 0 and 7), apparently because their natural feeding habits tended to be intermittent. Other rats might have been equally lucky, and chance evidently plays a part in determining the degree of success. In order to avoid further unobserved takes

TABLE 32. *Takes of plain wheat and poisoned rusk by marked rats*

	Body weights (g.)			*9/10 Feb. (wheat)*				*16/ Feb. (5% zinc phosphide in rusk)*			
										mg. zinc phosphide per kilo body weight	
Rat No.	/25 *Jan.*	9/10 *Feb.*	16/-/22 *Feb.**	*No. of visits*	*No. of periods in which recorded*	*Wheat take (g.) (0·455 g./visit)*	*Expected max. take (g.)*	*No. of visits†*	*Rusk take (g.) (0·429 g./visit)*	(*calc.*)	*obs.*
1	160	215	245	47	25	21	27	0	0	0	..
0	160	230	**265**	68	23	31	28	0	0	0	..
7	260	325	**360**	128	52	58	35	1	0·4	56	..
6	280	325	345	122	30	56	35	11	4·7	680	..
3	290	330	350	164	30	75	35	7	3·0	430	..
8	300	340	360	66	33	30	36	5	2·1	290	..
5	300	325	**340**	182	39	83	35	6	2·6	380	tr.
10	300	340	360	106	28	48	36	1	0·4	56	..
4	390	405	**410**	138	38	63	40	3	1·3	160	256
2	(190)	(245)	(270)	(9)	(6)	(4)	(30)	24	10·3	1,900	..
9	240	280	**300**	103	45	47	32	21	9·0	1,500	1,650
Mean‡	268	312	334	112·4	34·3	51·2	34	7·4	3·2	490	..

* Observed weights are in heavy type; the others are corrected as explained in Appendix 2.

† In addition there were two visits by unidentified marked rats.

‡ Omitting rat No. 2 before 16/ Feb.

during the night, the poison was removed at 10·15 p.m. after 2 hours during which only two mouthfuls were taken by unmarked rats. Forty-five grammes of wheat were taken later that night and 100, 36, 25, and 45 g. on the next four. The high take of 100 g. probably represents feeding by rats which were dying slowly because of their relatively low poison take and which did not appear in the hour during which postbait takes were being watched on 17 February. That evening and the next (7–9 p.m.) No. 0 was feeding well (Fig. 14). No. 7 was seen the first evening when it showed great suspicion of the tray which it refused to approach; it fed five times the second evening.

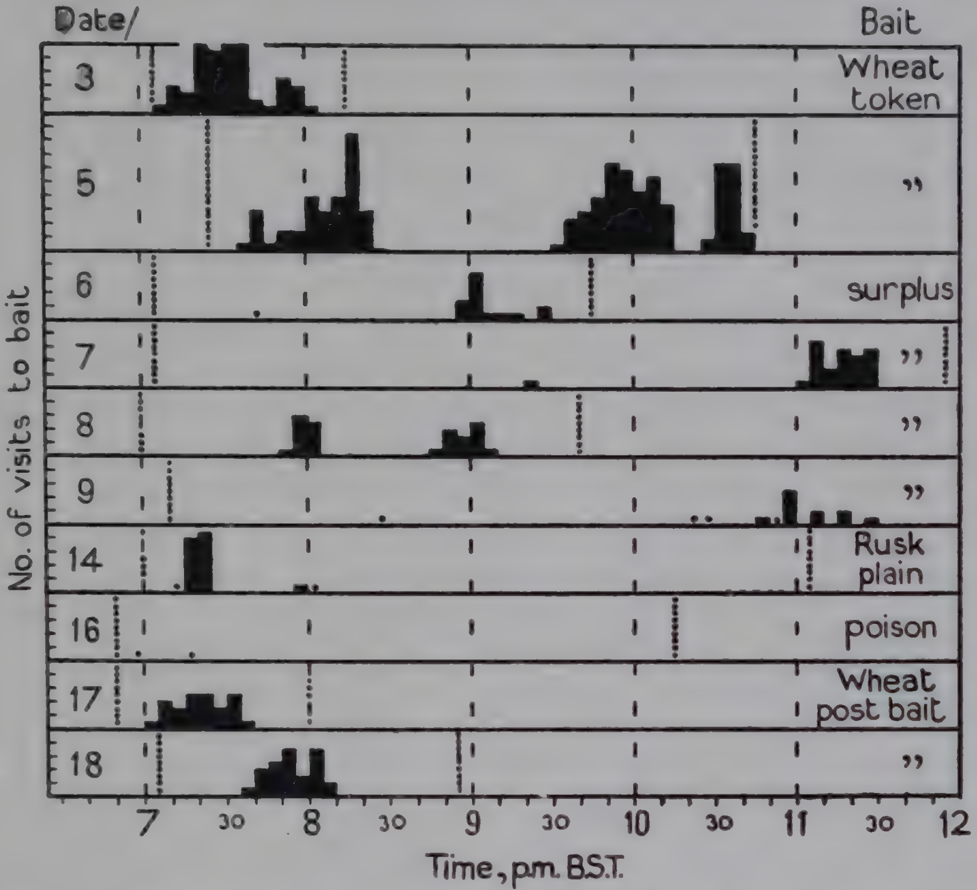

FIG. 14. Distribution and frequency of feeding visits by rat No. 0, 3–18 Feb. 1944. The height of each panel represents ten visits, or twenty on 5 Feb. Vertical dotted lines show beginning and end of watching; single dots show appearances without feeding.

In order to test whether the postbait takes really did represent these two rats only, a number of gin traps was set. Nos. 7 and 0 were caught on 22 February and all holes were blocked. There was one small hole open on 23 February, but otherwise no sign of activity and no postbait take. It is therefore assumed that this test was about 90% successful—two survivors out of about twenty rats.

(iii) *Discussion*

The activity pattern of the white rat is discussed in Calhoun's review (1945–6) which should be consulted for additional references. White rats have a well-marked nocturnal activity period except in their early months and in old age (Slonaker, 1907). Among brown rats age-specific differences seem to have been shown on an occasion when several young and one very large rat were the only ones which fed freely by day in the open air; most of the medium-sized rats stayed under cover and obtained their food by turning the young over on their backs and taking their food away when they returned. As a rule, however, rats of all ages could be attracted to baits in the day-time and, as among white rats and mice, a nocturnal rhythm can become secondary to patterns set up by restricted food supplies. White rats may have a peak of activity just before the time food is due and brown rats seem able to anticipate the arrival of token baits after a day or so.

Normal feeding rhythms have been studied by Richter (1927) who suggests that contraction of the stomach initiates restlessness and later food-seeking. Calhoun (1945–6) has since pointed out that contraction and activity may both be initiated by a common factor. The periodicity of the feeding rhythm of the white rat is given by Richter as 3–5 hours, but we cannot judge how strictly this is maintained as the chief data (for four rats) are given only as a curve covering the 24 hours. The intervals between feeds had a mode at 4 hours but varied from about ¾ hour to 14 hours, the longer intervals, it was believed, being identified with periods of sleep. In strictly nocturnal individuals the periodicity would no doubt have been modified. It is also possible that the frequency of intervals between feeds is not the best way of presenting the data. An underlying rhythm, for example, becomes obliterated in the analysis of such records if the individuals emerge for an occasional mouthful between their main feeds.

Our own observations, as already explained, were affected by the rats' abnormal appetites and by the noise of the wind and rain. There is a suggestion, however, that although individuals were extremely variable, some did tend to eat the bulk of their food periodically, with irregular visits between. In a natural environment it would be surprising if complete regularity were shown. Irregularities in feeding rhythm were also caused by mating (Appendix 2). The oestrous cycle of the albino rat has been re-examined by Blandau, Boling, and Young (1941). The length of the cycle is about 4 days with extreme variations of 2–19 days and heat lasts about 14 hours or rather less if copulation occurs at the start. The onset of oestrus is usually between 4 p.m. and 10 p.m., or soon after the end of an artificial 12-hour day (Farris, 1942). Greenman and Duhring (1931) describe the mating behaviour as follows: 'During coitus the female throws her head back, her tail forward and arches her back in a concave position. The male may make several unsuccessful attempts at coitus, following which he usually rolls back on his hips and licks his genitals. Following a successful coitus, he usually rises on his hind feet and makes no immediate further attempt with the female. None of these actions, however, are positive indication of a successful coitus.'

Young (1941) observes that the movements of the female are 'quick and darting, often in hops accompanied by a quivering of the ears or the entire body, and the female does not run away from the male, except that following a moment of smelling or licking by the male she runs forward and halts where she is usually overtaken and caught. . . .' Copulation, real or attempted, is frequent and brief (1–4 sec.); references are given by Reed (1946) and Beach (1947). Miller (1911) has observed similar behaviour in captive brown rats.

Behaviour at the experimental colony was precisely as described above. No males were seen which failed to lick their genitals for a protracted period after each mounting. This licking was also done on 6 February even though no coitus occurred. On this date the female was presumably in earlier oestrus than on 14 February (Appendix 2) or just past it.

We may conclude that the natural variability of the feeding pattern, together with that caused by disturbances from weather, fighting (Table 20), mating, &c., reduce the chances that all rats will take a lethal dose of poison. The object of token baiting is to lessen this variability, and the present observations show how this may be achieved when natural food is scarce.

(*b*) **Speed of feeding on different bases**

In order to increase the intake of a bait it seems necessary to provide one of which larger or more frequent mouthfuls are taken or which is so attractive that the animals continue feeding for the maximum time. In the experiment just described there was little difference in the size of the mouthful of wheat and rusk; but in other cases a larger mouthful may be taken of a wetter bait. Table 33 shows that while an average mouthful of wheat rarely exceeds 0·5 g. there were occasions on which the same rats took much more soaked wheat or rusk.

TABLE 33. *Size of mouthful*

Test	*No. of days observations*	*Wheat*		*Rusk*		*Remarks*
		Take (g.)	*g. per visit*	*Take (g.)*	*g. per visit*	
A	2, 1	1,150	0·446	60	0·429	Feb. 1944. 1 : 1 rusk + zinc phos.
B	1	80	0·494	. .	. .	See Chitty and Shorten (1946)
C	1	120	0·272	. .	. .	. .
D	4, 3	189	0·415	650	1·113	2 water: 1 rusk
E	2, 5	250	0·388	965	0·488	1 : 1 rusk
	1	. .	. .	90	0·450	1 : 1 rusk + 10% arsenic
F	4, 2	460	0·451	240	1·263	Caged rats; 1 : 1 rusk
	2	290	**0·749**	. .	. .	**Soaked** wheat

Tests A to E were on different populations at the experimental colony.

Another experiment on rate of feeding was made by finding the take of wheat and rusk within ½ hour from the start of token baiting. Some error is to be expected where individuals cannot be identified as no correction can be made for differences in the number of animals present or for delays between laying the bait and the start of feeding. In this experiment, however, the rats were highly conditioned and there was normally no delay before feeding began or reached a certain intensity. There were no known disturbing factors except on day 7 when rough weather may have lowered the feeding rate. Baits were given daily and results are shown in Table 34.

TABLE 34. *Total weight (g.) of dry or wet base taken in ½ hour at the experimental colony*

Day:	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*
Wheat	210	255	. .	. .	. .	. .	155	265	205	210
1:1 rusk . . .	. .	. .	170	330	390	475	. .	. .	. .	. .

There is little doubt that after the first day of the change to rusk the population was eating at an appreciably greater rate than before. In the space of half an hour it is unlikely that any individual became surfeited on wheat or rusk, so that each rat must have spent about the same total time feeding on each bait. Therefore either the mouthful was about twice as large or else the rats ran in and out twice as often; and watching showed no obvious difference in activity. The gradual increase in speed of feeding on rusk confirms the value of prebaiting, and it is possible that the low feeding rate on day 7 was really caused by the change back to wheat rather than by the weather (cf. Fig. 10, day 27).

Evidence from takes of wet and dry poison baits supports the view that the former are eaten more rapidly (Ch. 7). Poison takes average about 10% of the take of wheat when a dry bait is used and considerably more with a wet bait. With either type of base there is probably the same short time only for feeding, so that the difference must be due to a difference in size or frequency of the mouthfuls. Occasionally the difference may have another explanation: that under conditions of water shortage feeding may finish sooner with a dry bait.

(*c*) Choice tests

The study of attractive bases has often been carried out by means of choice tests but without much regard to the difficulties of interpreting them. With two baits of equal calorific value it is a safe assumption that if one is taken in greater amounts than another it is the more acceptable bait. However, other factors than weight of take are important when such dissimilar baits as wheat and bread mash, or flour and cabbage are being considered.

Rubner's isodynamic law states that under certain conditions foodstuffs may replace each other according to their heat-producing value. The exactness of this principle has been questioned by a number of authors who show that it must not be interpreted too strictly. There are also complications due to 'specific dynamic action'. Thus protein has a very considerable effect in stimulating metabolism whereas fats and carbohydrates release energy without using up very much in their own metabolism. (For discussion and references see Bodansky, 1938.) A simple illustration follows.

When one caged rat is eaten by several companions (as a rule only the skin being left) there is comparatively little effect on the wheat take per living rat. This is evidently because the rat has a fairly high water and protein content. The Medical Research Council (1945) quotes the composition of a skinned and gutted rabbit as 72% water, 20% protein, and 6% fat. A 100-g. rabbit, skinned and gutted but including the bones, yields 111 cals. Boas-Fixsen and Jackson (1932) quote figures for 'whole rat' from which the water and protein content appear to be of the same order (1 g. dried lungs, liver, heart, kidney, and muscle are equivalent to 3·2 g. fresh rat and contain 10·37% nitrogen or about 65% protein).

The common British cereals are fairly similar in their chemical composition and it may be expected that one will replace the other in the rats' diet more or less according to their calorific values. However, as shown in Chapter 7, there may be little difference whether the relative food values are compared on this basis or that of dry weight of cereal. The composition of 'dry' rusk is given in Chapter 7 and that of other foods by the Medical Research Council (1945). Other information is given by McCance and Widdowson (1942), and from Woodman (1944) the feeding value of maize can be compared with that of wheat and barley; these three baits were important in rat census work in the Middle East (Ch. 14). Calculated from digestible nutrients, maize has a starch equivalent (when fed to sheep or cattle) of 77·6 pounds per 100 pounds grain compared with 71·6 for wheat and 71·4 for barley. Platt (1945) gives a value for maize of 349 cals. per 100 g. Though we have no data on the cereals actually used in the Middle East these figures suggest that little error will be introduced by comparing takes on the basis of cereal content. It appears that no attention has hitherto been paid to the energy value of foods used in rat baits, and though the subject must be approached with caution, it has an important bearing on the interpretation of field results.

Difficulties about measuring preference may be understood more clearly from considering how to interpret the choice test in Table 35. For the purposes of discussion this may be assumed to have been a field test, though it was actually carried out with nine caged rats. Similar examples have been obtained in the field but are less illustrative. Several pairs of baits were offered in alternating positions, and the take, expressed in calories, remained fairly constant throughout. A possible exception was with soaked wheat when total take was consistently low for the 4 days.

This is characteristic of some field tests (Ch. 7) and is undoubtedly because the softer bait is eaten with less waste than dry wheat (§ 6 (*a*)). Carlson and Hoelzel (1949) find that soaked maize is not wasted to the same extent as dry maize. The rats' preferences are quite obvious for the first few pairs: soaked or boiled wheat was greatly preferred to dry wheat; rusk or dry wheat to sugar-meal. It is in interpreting the takes of rusk and dry wheat by themselves that the difficulties arise: (1) does the selection of a greater weight of rusk suggest that it would be more suitable for poisoning, or (2) does the superior calorific value derived from wheat suggest it as the better competitor against the normal food supply? (3) Is one base eaten more rapidly than the other? (4) Which is eaten first? Not one of these questions can be answered, but it is worth discussing the first in more detail.

TABLE 35. *Choice tests with nine rats in an outdoor cage*

	Daily takes (*g*.)						*Daily takes* (*cals*.)					
Dry wheat	90	80	70	70	60	70	297	264	231	231	198	231
Boiled wheat	300	330	..	..	..	..	550	606	..	..	..	..
Soaked wheat	..	..	200	210	230	190	..	..	482	506	554	458
Total	..	..	..	..	..	..	847	870	713	737	752	689
Dry wheat	..	..	230 - - -	230*	220		..	..	759	726		
Sugar-meal	40	70	30 - - -	30*	40		139	243	104	139		
1:1 rusk	380	400	..	..	..		618	650	..	..		
Total	..	..	..	..	..		757	893	863	865		
Dry wheat	160	120	160	140			528	396	528	462		
1:1 rusk	180	210	200	230			292	342	325	374		
Total	..	..	..	..			820	738	853	836		
	100 g. wheat increased to 137 g. after 24-hour soaking; to 180 g. after 1-hour boiling						Calorific value per g.: wheat, 3·30; sugar-meal, 3·47; dry rusk, 3·25					

* Average takes.

If more of one bait than another is eaten per unit time of feeding it will probably be the better one to use with poison. However, even rats with abnormally high food requirements spent at most 4·3 hours of the night eating wheat (rat No. 7, Table 32). It would thus be unnecessary for a rat to feed more rapidly to consume a much larger quantity of some other bait: longer time could be spent in feeding at the same rate. Adolph (1947), by diluting the food of white rats, has increased the duration of their daily feeding from 1·2 to 6 hours.

A choice test would be valuable if total take showed which of two bases the rats took more eagerly when they began feeding, and tests confined to the first half hour or so of the night might well be carried out. At present no information of this sort can be derived from the figures. In the outdoor cages, for example, large amounts of cabbage may be eaten compared with wheat (§ 7); but token baited rats took no cabbage at all until the last grain of wheat had disappeared.

A practical example may now be discussed. As shown in Table 52 (*b*) zinc phosphide is less successful when used with flour than with sugar-meal. Choice tests suggest that these results are probably explained by a greater eagerness of the rats for the sweeter base. Middleton offered two wild colonies the choice between plain flour and 12½% sugar-meal (Table 36). Positions were altered daily in a choice box holding six tins, and sugar-meal was taken in far greater amounts. An exactly similar test elsewhere showed that 5% sugar had less influence on the choice, so that a higher concentration was evidently needed; 10% was selected arbitrarily.

TABLE 36. *Simultaneous takes* (*g.*) *in two choice tests*

	Base *Day:*	*1*	*2*	*3*	*4*	*5*
A	Flour	35	25	35	5	30
	Sugar-meal (12½%)	115	150	460	535	600
B	Flour	180	110	170	..	..
	Sugar-meal (5%)	200	260	250	110	..
	Sugar-meal (12½%)	..	..	..	380	..

Two tests were done in outdoor cages with 15% medium arsenic in the two bases. While both were about equally successful (Table 37), the take of poison bait was relatively higher with 10% sugar-meal and the rats died within a day. Some of those which had taken plain flour lived until the seventh day and it was concluded that the sugar had accelerated the rate of intake by the other rats. While these experiments offered an explanation of the field results the latter could not have been predicted from a choice test alone: for sugar-meal itself when offered with certain other choices (e.g. Table 35), does not seem to be a promising bait.

TABLE 37. *Results with arsenic in different bases* (*caged brown rats*)

Base	*Peak take of surplus prebait* (*g.*)	*Take of poison bait*		*Mortality*
		(*g.*)	*% peak prebait*	
Plain flour	400	20	5	10/11
10% sugar-meal	385	30	8	12/12

Surplus choice tests, then, cannot do more than show the acceptability of one base in relation to another whereas it is against the natural food only that a base has to compete in practice. Failure to appreciate this point may result in discarding some quite simple bait and using one which is more expensive or inconvenient.

There are, however, at least four ways in which choice tests are useful.

Eliminating useless bases. If two or more baits are offered which in total amount are small in comparison with the feeding capacity of the population, then those which are not entirely eaten are obviously unsuitable bases (e.g. Coogan, 1945). Such tests with several baits are of particular value when the rats have strongly established food preferences. They are otherwise of little value since the range of acceptance usually covers all the normal classes of baits.

Finding a census bait. With certain baits entirely eliminated, the remainder may then be tested in pairs by surplus baiting. Since the object of a census bait is to supplant as much as possible of the rats' normal food it may generally be assumed that the bait from which most calories are derived will prove the best when offered by itself. This assumption is not valid if the less favoured bait supplies certain essentials without which the other could not be eaten as freely, as when the water requirements are met by one bait and not from any other source. The apparently better bait should therefore be continued in the absence of the alternative and the calorific take compared with the previous combined total. The method of offering different bases in succession is described in Chapter 7.

Minor modifications of a base. Natural food conditions may be such that even the most successful census bait does not adequately prebait the population, or for practical reasons it may be decided that one of the less favoured bases must be used. The attractiveness of such bases can sometimes be increased by the addition of other food substances, mostly forming a relatively small proportion of the total weight. It may also be necessary to test the palatability of an ingredient needed as a colouring agent, adhesive, mouldicide, &c. In such cases palatability can be judged from the relative consumption in grammes, and differences in calorific values can be neglected.

Investigating base-shyness. The relative take of two acceptable bases is found before and after a poisoning in which only one of them is used. A subsequent drop in the relative take of the latter is likely to be due chiefly to base-shyness.

A few examples from such tests will be described briefly. (*a*) Hard Manitoba wheat was tested as a census bait and was taken poorly in a choice with English wheat. Nevertheless the take of Manitoba wheat when offered by itself was greater than the combined take of the two wheats together. The anomaly that the least popular bait apparently drew the rats more successfully from their natural food is probably explained by a greater waste of the hard wheat (Table 38). (*b*) It was discovered accidentally that one sample of English wheat was preferred to another. Some rats in an outdoor cage were being offered flour in one pan and wheat in another. On the fifth day a different sample of English wheat was offered for one day and proved far more attractive (Table 39 A). Another test intervened, then the new wheat again proved more attractive (B). The chief apparent difference in the preferred wheat was a higher content of broken grains. This test shows that the same supply of wheat should be used in any one census. (*c*) There is no explanation for some of the daily variations in take

TABLE 38. *Takes (g.) of hard and soft wheats offered together and in succession to wild rats*

Day:		*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*
Test A	English	150	100	240	70	165	138 (av.)		160	270	240	..	..	..	..	150	350
	Manitoba	0	25	0	35	0	22 (av.)		40	70	120	410	505	530	660	35	45

Day:		*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*
Test B	English	..	..	..	..	..	..	820	870	820	..
	Manitoba	1,150	790	1,300	900	1,220	680	..	..	..	..

TABLE 39. *Proportion of flour and two samples of wheat in the diet*

A	Take (g.)	Wheat	250	270	250	250	**390**	180	140	200
		Flour	292	272	232	217	82	262	267	280
	Take (cals.)	Wheat%	31	49	51	53	**82**	40	34	41
		Total	1,827	1,824	1,621	1,569	1,568	1,493	1,378	1,620

B	Take (g.)	Wheat	165	**370**	315	350	310
		Flour	265	90	180	205	185
	Take (cals.)	Wheat %	38	**80**	62	62	62
		Total	1,453	1,530	1,657	1,858	1,658

Calorific values per g.: flour, 3·43; wheat, 3·30.
Heavy type: different sample of wheat.

in this last experiment, nor for the shift to a proportionately higher take of original wheat on the last 3 days. The food pans, however, were not redistributed each day at random as they might have been to allow for place preference, or sudden or progressive changes in it. These difficulties were illustrated by the following cage test (Fig. 15). Wheat was offered in pairs of adjacent trays; north, centre, and south. The north pair was the most favoured until the approach was partly blocked by moving two nearby tins. Over the next 6 days the rats gradually shifted their attention to the south pair. Finally, this approach was partly blocked using an object newly introduced from outside. Whereas the moving of a familiar object had produced a gradual change, this time the rats reacted so strongly that they took no food from the south pair at first. (*d*) Aniseed has long been considered an efficient lure but as far as can be judged without any more solid evidence than the type of observation made by Broad (1813) who wrote 'they appear to be very highly gratified by the perfumed straw, and . . . will be seen to move about it with much apparent pleasure'. Our own preliminary observations confirmed that aniseed was not attractive (Boulenger, 1920; Barnett and Spencer, 1953).

(*d*) Literature on bait preferences

As Ward (1943) has stated 'rats vary so much in food preferences that any list of baits in order of average acceptance will mean very little. . . .'

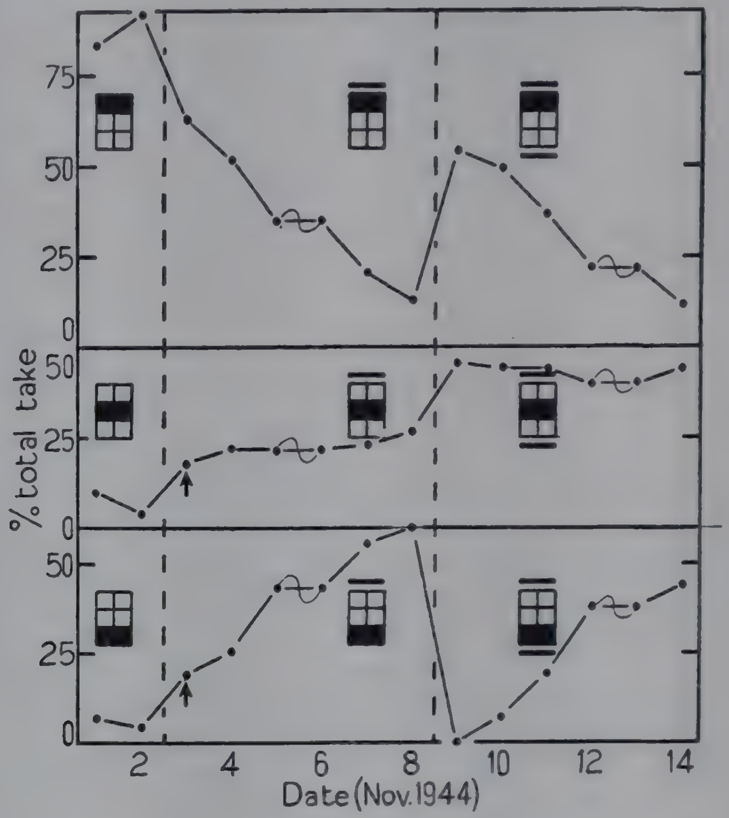

FIG. 15. Changes in place preference among caged rats feeding from a group of six trays. Days 1–2: free access to all trays. Days 3–8: limited access to north pair; days 9–14: limited access to south pair as well. Arrows show lack of a surplus, wavy lines show average takes.

Nevertheless many claims are made for the superiority of one bait over another and the general confusion of thought is well brought out by Mills (1942). Garlough (1943) has proposed giving attention to the dietetic balance of a poison bait, thus crediting the rat with better ability to recognize the long-term value of the base than the immediate consequences of the poison. Many studies of bait preference are invalid because of failure to take account of differences in moisture content or calorific value, or because they were carried out solely on caged rats (e.g. O'Connor, Buck, and Fellers, 1935). In other cases baits are recommended in the complete absence of experimental evidence. Two papers, however, merit very careful study as they describe methods which could profitably be combined and modified.

Crabb and Emik (1946) tried an interesting approach to the study of bait preferences among wild rats on Guam. Feeding stations were baited daily for 3 to 5 days with 4 oz. of each of four baits, one of them always being rolled oats. Analysis of variance was used to find out if acceptance of three test baits differed significantly from that of the standard. Since some of the 4-oz. baits were entirely eaten the total takes could not be used directly but were expressed as percentages and then converted so that the data fitted a normal curve.

Certain baits were eliminated in their first set of experiments, which also showed that the addition of fresh ground copra to a cereal bait increased the acceptance, a copra and oats mixture being taken most readily. In another group of tests with one part of copra to four of oats, acceptance was significantly better than that of rolled oats—plain or mixed with an oil, e.g. 1 part of corn oil in 15 (6·7%). In a third set of trials different mixtures of copra and oats were tested against the standard. Mixtures with 14 or 20% copra were taken significantly better than the plain, but there was little difference if only 10% were added.

It is not easy to evaluate these results. Platt (1945) states that the water content of ripe fresh coco-nut kernel varies from 20 to 52%, but he gives a fairly high representative value of 404 cals. per 100 g. The higher takes of the 14 and 20% mixtures with oats may therefore represent a real preference rather than a higher take due to greater moisture content of the bait as compared with plain or oiled oats. Even so these results do not necessarily indicate that these mixtures would be superior for use with poison, nor can the baits tested in their first series be rejected solely on the basis of acceptance relative to rolled oats.

Crabb and Emik observed that on the last 2 days there was seldom much difference in acceptance whatever the bait and that 'it seemed that the most selective discrimination was exhibited during the first two days. . . .' They also found an increase in total consumption throughout their tests. Now it seems likely that these two observations are connected: for with increasing numbers of pans completely finished less choice would be available. It is probable that the technique should be modified for use with a surplus of each bait.

Choice tests with surplus prebaiting have been tried by Doty (1945). Stations were placed in pairs every 50–70 ft. with 4–6 ft. spacing within pairs. The significance of the differences in take between pairs was tested by Student's method, and in dummy trials the chance differences between odd and even numbered pans proved to be very slight.

Comparisons were made in pairs between plain and oiled oats and between oats with different oils or colouring agents (1945, pp. 167–75). In these tests 30–60 pairs of stations were generally used, takes for the whole period ran as high as 17 kg. and gave very clear-cut results. It will be noted that the baits were of almost indentical food value and that therefore certain inferences can safely be drawn. For example where an oil, a dye, or some other chemical had to be added, these choice tests showed which substances or concentrations were acceptable. It is incorrect to assume, however, that an increased take of treated oats at the expense of plain proves the necessity for adding corn oil (or methylene blue which also improved the acceptance).

Similar choice tests were run with pairs of poison baits and showed, for example, that in low concentrations zinc phosphide was as acceptable as thallium sulphate; but as each poison bait could be visited by the same rats relative efficiency was better tested by a different experimental design (1945, pp. 186–9).

(*e*) Discussion

This section has dealt extensively with observations on the feeding behaviour of wild brown rats since this subject is the key to a proper understanding of control by poison and has almost entirely been neglected in the past. Doty (1938, 1945), Minckler and Pease (1938), and Coogan (1945) have recorded a few observations on natural behaviour towards baits, but most ideas are founded on the reactions of domestic stocks.

A figure of 1 g. is sometimes quoted as the amount of bait which a rat may be relied upon to eat, and concentrations of poison are worked out on this assumption. Schwartze (1920) appears to have originated this legend, though most of his ideas about the use of poison have remained in advance of much that has been written since. 'For practical purposes the individual food intake is the most important since the rats which eat scantily are the ones which necessarily determine the percentage to be placed in the diet.' However, he concluded that an intake of poison bait equal to about 1% body weight could be expected; hence that a 20% barium carbonate mixture would be highly efficient. His main data were the intakes of a concentrated laboratory diet by white rats after 12–24 hour starvation; but he pointed out that a moister food would be a better base.

Munch (1928) states that on the basis of previous experiments ½–1% body weight of food is usually eaten in 10–30 minutes, and the limits are narrowed to 1% in 15–20 minutes by Munch, Silver, and Horn (1930). Hence it is argued that if the M.L.D. of squill is 1,000 mg. per kilo death will occur from a 10% mixture eaten at the stated rate. Some brown rats were used in this latter study, but they had been fed for a week on the base to be used with poison and were starved 18 hours before the test. Garlough (1938) stated that many previous experiments had shown that at least 1 g. poison bait was eaten by a hungry rat. Coogan (1945) regards a 1-g. intake by wild rats as a 'nibble'.

It is probable that the enormously variable intake of poison bait by wild rats has not been suspected from the relatively uniform response which a good laboratory stock is expected to give. In the wild, a rat is confronted with an unfamiliar bait, is subject to disturbance, and has not been starved. Moreover, the natural population will probably have a wider range of susceptibility than the laboratory stock, so that a very considerable margin of safety is needed.

The conclusions from our own watching experiments are confirmed and much extended by data on stomach contents (Ch. 9). The minute intakes of bait by some of the marked rats are shown by Rzóska to be a feature even of those relatively successful campaigns in which average intake is high. Moreover, neglecting all animals (sometimes half the sample) which took minute doses, Rzóska shows that an average intake of 1% body weight is not invariable even after prebaiting.

The three lines of evidence—observation of live rats, stomach contents of dead ones, and the empirical data of Chapter 7—therefore demand a revision of ideas about the response that may be expected from a significant proportion of a wild population. In the latter part of this section it is shown that the usual method of choice testing in order to find efficient bases is of strictly limited value and that incorrect inferences are sometimes drawn from the data. Some of the legitimate uses of the method are indicated; but, at present, the only proof of relative efficiency of two acceptable bases is by field testing with a poison.

16. ENVIRONMENTAL AND OTHER FACTORS

(*a*) Age of poison bait

As explained in Chapter 2 certain dry baits containing fine arsenic proved unpalatable after storage, and any surplus from one operation was therefore discarded. When medium arsenic and zinc phosphide became available experiments were made to see if a similar waste could be avoided. The experiments were carried out by Ranson and Thompson, and a full statistical analysis of the laboratory tests was made by Leslie. The following results apply only to 10% sugar-meal and not to wet baits, which soon become mouldy.

Feeding experiments were carried out on white rats with baits of various ages after storage in tins exposed to the air. Starvation for 24 hours preceded each test. Preliminary experiments with a medium grade of refined Canadian arsenic had strongly suggested that baits 10 and 16 days old were more palatable to males than fresh or 21-day-old baits and this result was confirmed in a larger test on females with week-old bait. There was little difference between the means for 0- and 21-day-old baits but the 7-day-old bait was significantly more palatable. After 6 weeks' storage takes were less satisfactory. The same stocks of sugar-meal and arsenic were used in each test. In a parallel experiment in which fine American arsenic was used in baits 0 and 21 days old the latter was poorly taken and failed to kill any rats (Table 40).

The experiments show that these baits deteriorate, the speed evidently depending on the particle size of arsenic. In similar tests with zinc phosphide baits 0, 7, 14, and 21 days old there was no correlation between take of poison bait and its age.

TABLE 40. *Results with arsenic and sugar-meal after storage*

Particle size	*Concentration* (%)	*Age of bait (days)*	*0*	*7*	*21*	*42*	
Medium . .	15	Mean take (g.)	2·53	4·09	2·48	1·57	♀♀
		Mortality	10/10	10/10	10/10	8/10	
Fine . . .	10	Mean take (g.)	2·41	..	0·35	..	♂♂
		Mortality	3/3	..	0/3	..	

In the outdoor cages rats were killed with equal success by arsenic baits 1, 3, and 6 weeks old (mortality 19/21 animals in four tests) and by zinc phosphide baits 1 week old (mortality 19/21 animals in four tests). Two paired field tests of fresh and week-old arsenic baits and one test of the latter by itself showed that it was acceptable and efficient in containers. Zinc phosphide baits 3 weeks old were used in the field with hole baiting on the 1–3–5 system. In three tests all the rats were killed (Table 51 (*b*)). It was recommended that sugar-meal and 5% zinc phosphide might still be used when 3 weeks old; but lacking field evidence not more than 1 week's storage was advised for arsenic baits.

(*b*) **Operator**

Several members of the Bureau generally contributed to each series of trials and obtained similar results. There were individual differences in the way census data were collected, especially while we were working out the most efficient and economical methods. In time standards of procedure were evolved, and although the number and position of census points had to be decided on the merits of each case it seems unlikely that the personal factor influenced the results or the estimates.

After adequate instruction, non-scientific staff also carried out entirely reliable control work. Good results were achieved in a London factory, in the area of P.L.H.A., in various sewer rat campaigns, in a Berkshire farming area and elsewhere, and by householders in Oxford. A high standard was also maintained by the staffs of many W.A.E.C.'s and sanitary departments; but others undoubtedly failed to recruit sufficiently high grade labour or provide intelligent supervision. It also appeared that, after an initial interest and success in controlling some of the larger and more spectacular infestations, many operators became bored by the routine of maintenance treatments and failed to apply the skill and initiative essential in rat control. The results of a series of practical treatments by a county staff were measured by Miss Shorten and found to be exceedingly poor (Table 41). Some of these censuses were restricted and are therefore not fair samples (§ 4 (*b*)) but there seemed to be no doubt that the results were poor. With the methods and materials at present available for rat control there may, in practice, be many similar failures with control methods which are sound enough when carried out properly. Unless revolutionary technical changes are made (§ 19) there is likely to be no solution to this problem other than thorough training and supervision.

TABLE 41. *Results of farm treatments by a team of practical operators*

Poison bait	*Peak take of wheat (g.) in census 1*	*% kill*
S a	1,025	41
R a	600	0*
S z	1,230	99
R z	1,730	30
R z	2,445	44*

a = arsenic; z = zinc phosphide; R = rusk; S = sugar-meal.
* Restricted census.

(*c*) **Size of infestation**

Very large infestations present certain practical problems such as the difficulty of judging the right quantities of bait and how it should be distributed; but apart from the scale of operations results were much the same with different colony sizes. When containers or direct poisoning were used small infestations were perhaps the more easily dealt with; but these instances do not prove that rats in small colonies have a different type of behaviour from those in the larger ones.

There are certain theoretical difficulties about comparing results in colonies of different sizes. If it be assumed that a treatment has a certain probability of success then the expectation of killing 85% or more of a population will differ according to the number of rats in the sample. Other things being equal there should be more failures (<85%) with very small populations; but also more cases of complete kills. The comparison of results based on colonies of very different sizes may therefore be open to certain refinements which we are not yet in a position to apply.

When dealing with small populations left after a poison there is no evidence that they are harder to eliminate if the right changes are made in base and poison. It is possible that sometimes the survivors have peculiarities in feeding behaviour which will again protect them from taking a lethal dose; but the only evidence about this is exactly the opposite. A marked rat No. 0 survived a poisoning because of its intermittent feeding pattern; but after the elimination of the other rats it fed far more intensively than it had in the crowd (Fig. 14). Other factors, however, such as reduced competition for natural foods presumably might act unfavourably against second treatments.

(*d*) **Environment**

(i) '*General*' *habitats*

Most of the tests used in comparing the efficiency of the poisons were carried out in rural habitats. Part of the reason for this is that the prebaiting system in urban work had not been fully standardized until 1944–5 and several tests are believed to have failed because of the wrong use of containers, novel bases, or insufficient number of baits. Also, advantage was taken of the greater accessibility of infestations in Oxford to carry out a number of experiments not related to testing the efficiency of a poison. Finally, except in the Oxford market, it soon became impossible to find many rats in the city.

Middleton has shown how efficiently control can be carried out in typical rural conditions (Ch. 10), and in our experience the greater number of urban infestations have also been easy to deal with. Many consisted of a few rats in the garden, especially where poultry was kept. These and others in railway yards, cellars, shops, and on rubbish dumps have presented no difficulties. In one or two routine treatments it was generally possible to exterminate the rats completely, and in most cases the premises remained clear thereafter.

There was one outstanding exception among the generally simple types of urban infestations: a small retail confectionery which required constant retreatment. Premises of this sort may be extremely baffling to a practical operator who cannot carry out census work to distinguish between poor results and reinfestation. Our own attempts at control were complicated by a wide variety of experiments on bases, poisons, and baiting systems, but after as much as possible had been learnt the premises were generally cleared each time. It is certain that this shop became reinfested from a sewer, but the building was so antiquated that the point of entry was never discovered. I am indebted to Thompson for help in preparing the following summary of events.

For several years previous to 1940 this confectioner's shop had been infested with rats and two servicing companies had failed to eliminate them. The rats did considerable damage, eating overalls, chocolate, cakes, mince pies, flour, custard powder, and other stores, besides gnawing the woodwork of showcases and trays, chewing up paper, making holes in walls, causing plaster to fall from the ceilings, and distressing the staff. On one occasion an electric service was inactivated by rats gnawing through the cable.

The first inspection was made by the Bureau in May 1940, and about fifty rats were found. Several evenings were spent observing them, as they came out readily when the lights were left on after the shop was quiet. Poisoning with 20% barium carbonate in bread mash left very few.

Rats were again abundant by April 1941. Recent control measures by a servicing company had had little effect, nor were our own poisonings completely successful at this date. In an attempt to trap the rats with break-back traps three were caught, but the few survivors avoided the traps. In August 1941 a long series of experiments was begun which, because of our lack of knowledge about the principles of follow-up treatments, was not completed until February 1942. The peak take of wheat in the first treatment was 730 g. and the last of the rats were eliminated by 2% thallium sulphate in flour. There were no rats for 3 days after the thallium sulphate, then a few footprints appeared in trace patches and there were small takes of wheat. Six weeks later a few more rats had arrived and they were given 10% arsenic in bread mash. The premises were then free for 4 days, after which one or two rats came filtering in and were again eliminated by arsenic in bread mash. Occasional inspections of the shop from May to August 1942 showed the premises to be clear, except for an occasional track made by a visiting rat.

Reinvasion occurred in September 1942 (295 g. peak take of wheat), and in another series of experiments the population was gradually eliminated, the last two rats being trapped. Trace patches showed the premises to be free up to March 1943, but in August another treatment was necessary. Poisoning with zinc phosphide at surface points on the 1–3–5 baiting system killed most of the rats, all the rest being eliminated by 2% Antu on soaked wheat. Rats were not seen again until November 1943 and then only a few were present. Zinc phosphide killed most of them but left a few survivors.

From November 1944 to March 1946 intermittent treatments were carried out by the Oxford City Sanitary Department but reinvasion was frequent and in April 1946 the shop was said to be overrun with rats. At the beginning of May 1946 an intensive treatment was made by the Sanitary Department with the collaboration of the Bureau. The premises were prebaited with sugar-meal at forty-six surface points and the nearby sewer manholes with rusk. Poisoning with zinc phosphide resulted in a complete clearance of the shop. Rat-proofing was begun immediately after this clearance and through August 1947 no further rats had appeared.

This example shows that there is no great difficulty about killing rats in this type of environment, but that poisoning alone is not enough to give permanent relief.

(ii) '*Special*' *habitats*

In the case just described it was not difficult to prebait the rats because other food was to some extent protected. In some environments, however, the rats have ready access to high quality foods and are not readily prebaited. Freeman first undertook a study of this type of habitat and some of his work in buffer depots has already been mentioned (§ 6 (*b*)). He concluded that such rats might not enter containers and that soaked wheat was often the only known bait that could be used. Similar conditions to those in buffer depots were later found in certain food stores, for example in the Oxford market, a collection of shops under a common roof occupying about 1 acre in the centre of the city. For many years rats had caused considerable damage here, only partly checked by regular attention from a servicing company from 1935 to 1943.

In May 1943 the Bureau attempted to clear the market. As the population was large and scattered only a few census points were established to obtain the order or magnitude of certain infestations. Insufficient wheat was laid in some places and the figures (Table 42) therefore give a low estimate even of these populations. Dry wheat was entirely refused from the floor of a greengrocer's though rats passed right beside it. At other places rats had refused to enter P3s but took wheat from surface points.

TABLE 42. *Summary of results in the Oxford market, 1943*

Shop		*Peak take (g.)*			*Trace patches*	
		Census 1				
No.	*Trade*	*Wheat*	*Rusk (dry content)*	*Census 2 Wheat*	*June*	*Aug.*
9 .	Greengrocer	150	75	65	+	0
21 .	Fishmonger	. .	210	. .	0	0
24 .	Butcher	50	180	0*	+	0
44 .	,,	. .	60	. .	0	0
32 .	Greengrocer	200+	255	400	+	0
38 .	Butcher				+	0
79 .	,,	200+	300	160	+	0
80 .	,,	200		0	0	0
95 .	Grocer	75	45	35	+	0
123 .	Greengrocer				0	0
99, &c.	Mixed (fig. 16)	625+	765+	300	+	+
108 .	Lunchroom	. .	30	. .	+	0
131 .	Butcher	. .	120	. .	+	+
135 .	Greengrocer	0*		. .	+	0
. . .	Cellars	. .	60	. .	0	0
Total take . . .		1,500+	2,100+	960	. .	. .

* Rat(s) present but refusing dry wheat.

Prebaits of rusk were then distributed all over the market, a surplus for 3 days, then a token for 2 more. Surplus rusk was poorly taken in some places and on the fourth and fifth days token baits were entirely refused in certain butchers', grocers', and greengrocers' shops.

The peak takes of rusk (converted into dry content) give a second underestimate of the size of the infestation. As part of the population was capable of eating at least 1·5 kg. wheat, and as all the rats together took 2·1 kg. of a cereal they did not particularly like, they must have been capable of eating at least 3 or 4 kg. wheat per day. Damage to food and structures was certainly consistent with a population of at least 100 rats.

Poisoning with 10% medium arsenic at surface points produced twenty-five corpses, a relatively larger number for this poison, which supports the belief that the census was an underestimate. More than 50% of the rats seem to have been killed, but definitely less than in any previous attempts at controlling urban rats. However, during June, July, and August further treatments were carried out and by August no more than one or two rats remained in any shop except No. 99 and its immediate neighbourhood. This one shop continued to suffer damage but on a greatly reduced scale.

One year later most other shops were still free of rats, but a colony was living on butcher's waste in No. 38 and in time would no doubt have spread. At another shop (No. 19) rats came in occasionally from adjacent property but a cat was now able to keep these under efficient control. At the two badly infested places (Table 43) poisoning was carried out with 5% zinc phosphide on soaked wheat but only about half the rats were killed. In each case 4 days' surplus prebaiting was done, but at this period we did not attempt to intercept the rats between their living and feeding quarters. The baits in the grocer's did not attract rats from their normal food, and damage to eggs, flour, and bacon still continued. After the poisoning in No. 38 a choice between dry and soaked wheat showed that the latter was preferred despite its use with poison.

TABLE 43. *Takes of wheat (g.) in the Oxford market, July 1944*

Shop No.		*Peak take in census 1 (g.)*	*Individual takes in census 2*					
			Days: 3	*4*	*5*	*6*	*7 . . . 19*	
38	Soaked wheat*	450	. .	. .	100	105	185+ . . .	. .
	Dry wheat	. .	55	110	40	70	25	. . . 240
	Total	450	55	110	140	175	210	. . . 240
99	Soaked wheat*	200	. .	. .	100	85	. .	. .
	Dry wheat	. .	65	50	. .	. .	. .	. .

* Corrected for added water.

The populations did not get out of hand until the following year when the rats in the butcher's spread to an adjoining florist's. A census was carried out here in July and prebaiting on the 1–3–5 system was done using bread mash and arsenic with 100% success (Table 51 (*a*)). Rats from the grocer's had spread to several other shops and in September Miss Mary Boyle of the Infestation Branch of the M.O.F. tested the 1–3–5 system under these conditions.

A census was done using dry whole English wheat (Table 44). Soaked wheat would have been more efficient but was to be used in the poisoning, and for experimental reasons it was thought best not to accustom the rats to it or risk the effects on the postbait of possible base-shyness. After 6 days, baiting was discontinued in two empty shops (Nos. 120, 121, Fig. 16) where a large proportion of the rats was suspected to be living. Before this, little census bait had been eaten in the grocer's (No. 99) and damage to goods and structures had been reduced; but now the take of bait increased in No. 99 proving that the rats had previously been intercepted in the empty shops. The total take, however, was lower than before, showing that correct placing of the census points was essential if maximum takes were to be obtained. In one shop (No. 113) no census could be carried out though rat tracks passed right beside the wheat. Fruit, vegetables, and dessert powders were continually attacked.

TABLE 44. *Takes of census bait (g.) in the Oxford market, 12 September–19 October 1945*

Shop Nos.	*Census 1 (last 4 days)*				*Census 2 (peak)*	*Census 3 (peaks)*		*Trace patches*
						Dry	*Soaked**	
120–1	695	700	..	..	350	200	200	+
99	45	20	425	360	70	0	85	+
123	195	170	275	210	0	0	..	0
103–4, 154	160	180	190	265	105	0	..	0
Total	1,095	1,070	890	835	525	200	285	..

* Corrected for added water.

After a 2-day interval, prebaiting with soaked wheat was begun at seventy-eight points including a number of small colonies outside the area of the census. Medium arsenic on soaked wheat was laid on the fifth day and left overnight, after which census baiting was resumed. Results were disappointing, only one-half to two-thirds of the rats having been killed. After this second census a 1–3–5 treatment was again carried out, this time using sugar-meal and 2% Antu. A third census was then started and trace patches were also placed in those parts of the market where no census baiting had been done.

By now several places were free, but in Nos. 120–1 rats remained abundant, and the difference in take between the second and third censuses was again disappointing. At the conclusion of the standard census, soaked wheat was placed at two sites, and the rats, instead of being shy, took more of it than they had of dry wheat (Table 44).

The exact kill cannot be stated. The successful July treatment together with Miss Boyle's tests greatly improved conditions in the market; but results were much below the standard normally expected. The peak takes of dry wheat (1,095, 525, 200 g.) suggest reductions of 52 and 62% respectively, a gross total of only 82%.

(*e*) **Conclusions**

These experiences in the Oxford market are of particular value since they show that rats in certain types of environment are not easily killed by poison. Results were very uneven, including several outstanding kills as well as failures, and it must be supposed that there is a large element of chance about the results in this type of habitat. No doubt the feeding tempo resembles that of the marked rats which, during surplus baiting, fed at a fairly leisurely pace throughout the night and showed no special eagerness even when they first broke their day-time fast. Nevertheless, most shops were completely cleared of rats, none suffered serious damage after 1943, and results can be considered poor only when compared with those in the simpler environments.

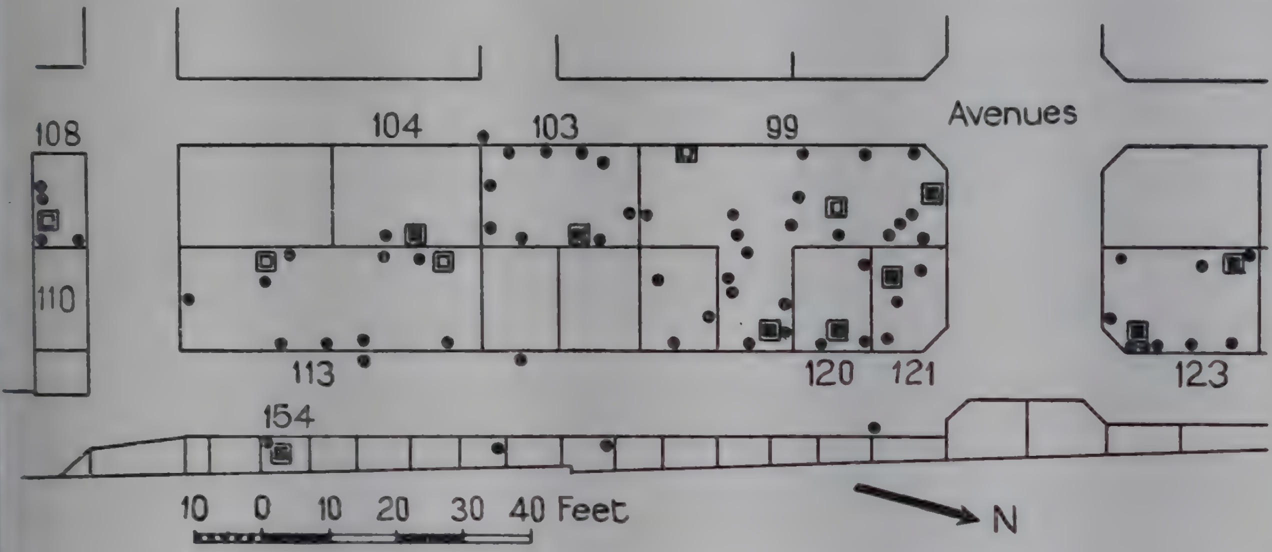

Fig. 16. Distribution of census and poison baits, Oxford market, Aug.–Sept. 1945. Numbers are those of certain shops; squares are census points, white centres indicating that little or no wheat was taken. Dots are poison baits on the ground, on shelves, &c.

Few of these latter presented special difficulties. In two Oxford groceries, which in type of goods and storage conditions resembled No. 99 in the Oxford market, containers were not entered and dry wheat or rusk was poorly taken in comparison with soaked wheat. In each case, however, 100% kills were obtained in a single treatment with this latter base. Another grocery was also largely cleared using rusk and arsenic and successes have occurred in various butchers', greengrocers', and other food shops. No special difficulties were experienced with rats in a variety of premises in London (Ch. 1, § 8) or with those in ricks or on well provisioned farms (Ch. 10) or in buffer depots (Table 45).

Difficult infestations may be commoner in other parts of Great Britain, and it would be idle to pretend that prebaiting can always be successful even in simple environments. However, throughout 1939–45 every available type was studied and our tests were as representative as we could make them.

TABLE 45. *Results of quantitative tests* with surplus prebaiting in buffer depots*

(*a*) White flour and sugar. Thirteen surface points. Flour patches showed no traces of survivors.

Soaked wheat:	85 – 85 – 85+	220	–	0	0
Soaked wheat and 10% fine arsenic:	– – – – –	–	57	–	–

(*b*) White flour and sugar. Six surface points.

Soaked wheat:	330+	380	385	211 –	212 –	212	–	0	0	0
Soaked wheat and 10% fine arsenic:	–	–	–	–	–	–	57	–	–	–

(*c*) Sacked oats. Ten surface points. The sacks were removed after the poisoning: one adult and four sub-adults were dead and four nestlings were alive.

Bread mash:	600+	180	225	250+	–	–
Wheat:	–	–	10	–	–	–
Soaked wheat:	–	–	–	85+	120+	–
Soaked wheat and 5% zinc phosphide:	–	–	–	–	–	< 30

(*d*) Sacked oats and sugar. Thirteen surface points. The oat sacks were removed after the poisoning: seven adults and five young were dead, none alive, though there were a few survivors in the sugar.

2:1 rusk:	495+	385	265	210	210	Gap 3 days, then:						
Soaked wheat:						650	700	680	740	710	–	–
Soaked wheat and 5% zinc phosphide:						–	–	–	–	–	< 130	

* Large-scale tests with token baiting were checked by other methods. + indicates that all bait was gone from one or more points. Hyphens show average takes.

17. BEHAVIOUR TOWARDS SUCCESSIVE POISON BAITS

(*a*) Introduction

Rat populations which have survived a poisoning often differ from untreated animals in their response to further baits. Tolerance may be built up so that even voluntary acceptance of a second dose may not result in death, and if the least susceptible animals have survived the first treatment results are likely to be worse next time. Some rats are able to acquire immunity to strychnine (Barnum, 1930; Dopmeyer, 1936; Cilento, 1936) and sodium fluoroacetate (Kalmbach, 1945); tolerance of Antu is quickly built up (Richter, 1946); brown rats can become accustomed to gradually increasing doses of arsenious oxide (Parfent'ev, 1927) and some of the species in Rangoon may have become resistant to barium carbonate (Harrison and Woodville, 1948). In addition, rats with unusual bait preferences or peculiarities of feeding behaviour may be relatively more common in the surviving than in the original population (Appendix 2).

We have no evidence to show the part played by these factors; but survivors may refuse or take very little poison bait when it is offered a second time, and may be said to be bait-shy. On other occasions there have been reasonably large takes of a previously experienced poison bait without an great reduction in numbers. Bait-shyness, therefore, may be partial o complete and may best be defined as all behaviour after experiencing a poison bait which favours survival of later treatments. Richter (1946) has studied shyness in one of the few experimental approaches besides that of Rzóska (Ch. 8).

Although it is recognized that rats become shy of some poison baits comparatively little attention has been given to the special problems of 'follow-up' treatments. Decker, Gunderson, and Barre (1941) recommend prebaiting for shy populations and recognize that best results are given by a change of base and possibly of poison. Garlough (1938) advises a change of base or poison. Eskey (1934) recommends treatments every 2 months using a different bait; but apparently all three of the best bases (rice, wheat, and barley) were to be used each time. Richter and Emlen (1946) advise using Antu with more than one base in the same treatment, and Silver and Garlough (1941) state that there is a better chance of success if poison is given in a choice of bases. Such procedure naturally limits the number of new baits available for later treatments.

In the field it is not always possible to prove that the results of a second treatment can be explained in terms of the previous use of poison. Survivors will have experienced sub-lethal doses of different strengths and others will have eaten no poison at all if its distribution was inadequate or the base did not attract them. Moreover, the rats which experienced the first poison bait will constitute an unknown fraction of the population, perhaps fairly small if several weeks elapse between treatments. Under these circumstances the effect of the first poisoning on the second is not directly measurable. Furthermore, census figures are hard to interpret in the case of small populations; whereas the larger ones are often those left in the more difficult environments and are therefore not random samples. Our own evidence will be treated briefly and its discussion will mainly be concerned with ways of testing preliminary conclusions from experiments with arsenic, zinc phosphide, and squill.

(*b*) **Results**

(i) *Field tests*

Where the same poison bait was used twice or more at the same locality some rats were generally killed each time, but it was obvious that the sites could not be cleared in this way. It will be sufficient to quote a single typical test with squill extract, a poison with which the chances of a successful follow-up seemed best (Ch. 2). Direct poisoning on a rubbish dump (with an alcoholic extract in rusk) had left a population which was then prebaited and poisoned on the fourteenth day after the original use of the bait. Only about 14% of these rats were killed, the takes of census bait being 1,670 g. wheat before and 1,430 g. after this second poisoning. Another type of evidence about the need for a change of bait was provided at localities which had been successfully cleared of large populations and then reinvaded within a few months. The first instance in Table 46 (*a*) occurred at a farm where sugar-meal and zinc phosphide were being used again within 6 weeks. This treatment failed, and since a dying rat had been found during the prebaiting period it seemed likely that the previous baits had been encountered by the incoming rats. Many of these baits had been placed within the dry walls of the buildings and had no doubt remained edible. Two other failures are probably explained in the same way.

TABLE 46. *Probable effect of old bait residues on results with later baits*

		1st poisoning(s)		*2nd poisoning*		
	Bait(s)	*% kill*	*Interval (months)*	*Bait*	*Peak take (g.) in census 1 or no. of rats seen (italics)*	*% kill*
a	S z.	95	1½	S z	*13*	46
	S z.	> 85	4½	S z	*16*	50
	R z.	> 97	5	R z	790	75
b	R a.	88	5	R e	750	29
	S z R a	few left	3	S a	675	68
	R e.	80	5	R z	570	15

a: same poison bait, *b*: similar poison bait.

TABLE 47. *Examples of bait-base-shyness among wild rats*

	*Subsequent peak take of plain bases (g.)**			
Poison	*Experienced base*		*Wheat*	*Bread mash*
a	K	75	164	118
a	K	40	80	120
a	K	30	75	65
z	K	90	210	23 (!)
a	S	25	150	. .
a	F	115	215	. .
a	F S	0 0	35	. .
z	F S	120 190	925	(Fig. 17)
z	R	55	250	. .
z	R	155	290	. .
z	R	2,035	4,730	. .
z	R	70	170	. .
p	R	0	120	. .
e	R	150	530	. .
a and e	R	190	535	. .

* Takes of rusk and bread mash are given in terms of cereal content.
For symbols see Appendix 1.

A frequent, though by no means invariable reason why the same bait failed a second time was that the survivors refused even the prebait.

Table 47 shows that flour, sugar-meal, or kibbled wheat, when offered to survivors from arsenic or zinc phosphide, was often taken in smaller quantities than normal (Ch. 7). Rusk, too, was often unacceptable after it had been used with zinc phosphide or squill; but as a rule neither rusk nor bread mash was avoided after use with arsenic or barium carbonate. Rats did not seem to become shy of whole wheat after it had been used, dry or soaked, with poison (e.g. Tables 43–44). An apparently normal take of such bases does not, however, ensure that they will be accepted with the original or even with a new poison.

TABLE 48. *Second treatments, with the same poison in a new base*

Interval (days)	*Container*	*Base*		*Poison*	*Peak takes wheat (g.) before and after 2nd poisoning*	
		1st	*2nd*			
45	P3	K	B	a	225	0
24	surface	K	B	a	75	0
59	,,	W	K	a	470	330
9	,,	W	B	a	375	310
12	,,	S	R	a	175	135
14	,,	R	S	a	120	100
11	P3	F	W	z	495	190
47	,,	F	W	z	925	490

For symbols see Appendix 1.

Few tests were done in which a new poison was used in the original base, for the risk that the rats may be shy of the base makes it necessary to use a different one in practice. Rather more attention was paid to using a new base as a disguise for the original poison (Table 48); but contrary to the evidence from white rats (Ch. 8, Appendix 1) very poor results were achieved. Since there was no practical need to study this question these preliminary tests were not extended; but together with certain failures which may have been caused by residues from former treatments (Table 46 (*b*)) they suggested that a completely different formula should be used. Table 49 shows that where this was done the results were good (they have indeed been included in Appendix 1).

In the tests so far considered there had been no more than one previous poisoning. In sixteen localities, however, three to seven treatments were carried out, mostly at short intervals and seldom without eventual success. We have no evidence, therefore, that rats can learn to avoid a poison bait which is sufficiently different from those previously experienced.

(ii) *Tests in outdoor cages*

Most tests in the outdoor cages (Table 50) were on a small scale because of the readiness with which the animals succumbed to their first poison bait; only at a later stage could reasonable numbers of survivors be guaranteed. The main results were as follows: (*a*) the same poison bait rarely killed any survivors from the initial treatment, an exception being that survivors from a low dose of arsenic were killed a long time afterwards by a normal mixture; (*b*) arsenic was readily accepted in rusk by rats which had survived it in flour or sugar-meal, but (*c*) when this sequence of baits was reversed only one out of sixteen rats was killed; (*d*) zinc phosphide killed three out of six rats which were shy of the poison in the original base; (*e*) on several occasions captive rats refused plain food with which they had received sublethal doses of poison. One instance is given in Fig. 17 to show how choice of food was affected by experiences with poison; (*f*) animals which had survived toxicity tests (Ch. 2, Appendix 1) were later tested in the outdoor cages for shyness. The original dose, given in a 1-g. pellet of flour and water, varied from 40 to 100 mg. arsenic per kilo body weight. The second bait was 15% arsenic in sugar-meal, which killed all but five out of forty-one rats. Thus the mere ingestion of arsenic does not induce shyness.

TABLE 49. *Second treatments, with a change of both base and poison*

(*§ 17 (a) explains the type of evidence presented*)

		2nd poisoning		
1st poison bait	*Interval (days)*	*Poison bait*	*Peak take (g.) last 3 days in census 2*	*% kill between census 2 and census 3*
R z	10	S a	295	100
R z	12	SW a	485	100
R p	28	S α	165	100
R p	27	S α	115	100
R p	14	S α	1,615	94
R p	14	S α	140	100
R e	25	SW a	245	100
R e	25	SW a	110	100
SB z	30	SW a	570	91
SB z	30	SW a	370	60
SB z	30	SW a	1,080	100
SM z	30	SB a	1,860	95
SM z	30	SB a	500	95
SM z	30	SB a	2,740	87
SW z	30	SB a	350	100

For symbols see Appendix 1.

In general these tests confirmed the field evidence that the same poison bait could not be used twice in quick succession and that the original poison could not always be disguised in a new base. A novel discovery was that arsenic was effective a second time if the bases were given in one order but not in the other.

(*c*) Discussion

Most of the field results so far discussed were obtained from animals which had survived by accident at localities where it was convenient to carry out further study; later on attempts were made to obtain larger residual populations. An obvious fault to be avoided was the uncertainty whether the rats had experienced or been missed by the original baits. Secondly, there had to be enough survivors and a reasonable certainty that they would not be diluted by too many untreated rats. This second condition limits most tests on the time shyness may last, for the only way of ensuring that there is little dilution by young and immigrants is to follow up quickly or to leave ample bait in the habitat. Yet a rat which always

TABLE 50. *Second and third poisonings of rats in outdoor cages*

Same base and poison			*Different base, same poison*			
	2nd poisoning					
Poison bait	*Interval (days)*	*Mortality*	*Interval (days)*	*Poison bait*	*Mortality*	
(*a*) *Normal concentrations throughout*						
S a	22	0/2	..	..	..	
F a	23	0/2	..	..	..	
F z	24	1/1	..	..	..	
R α	41	0/1	..	..	..	
S a	50	0/2	..	..	..	
F a	..	..	19	R a	1/1	12/12
S a	..	..	25	R a	2/2	
S a	..	..	51	R a	2/2	
S a	34	0/1	15	R a	1/1	
S a	12	0/6	7	R a	6/6	
R a	12	1/9	7	S a	1/8	1/16
R a	13	0/8	6	S a	0/8	
S z	15	0/3	7	R z	2/3	3/6
R z	15	0/2	7	S z	1/2	
R z	12	1/2	7	S z	0/1	
S a	10	0/2	..	..	..	
S a	32	0/2	..	..	..	
Total	..	3/43	..	..	16/34	
(*b*) *Low concentrations in first treatments* (*only*)						
R a (1%)	74	5/5	..	..	..	
R a (1%)	74	4/4	..	..	..	
S a (1%)	27	0/4	..	..	..	
W a (2%)	3	0/1	7	B a	1/1	
S a (2%)	..	..	8	SW a	1/1	
R e (½%)	15	0/1	..	..	..	
R w (½%)	13	2/3	..	..	..	
R e (1%)	15	0/2	17	B e	0/2	
Total	..	11/20	..	..	2/4	

For symbols see Appendix 1.

comes in contact with the same bait will presumably retain its shyness more completely than one which meets it briefly.

Prebaiting and poisoning by standard methods seldom leave more than 15% of a population, so that to get a 250-g. postbait at least 1,670 g. wheat must be taken before the first treatment. If large populations are scarce it is therefore necessary to ensure that the first treatment is not efficient. This can sometimes be achieved by direct poisoning, but this method cannot be relied on as about 40% of our tests were successful with zinc phosphide and Antu. It is possible to reduce the concentration of poison so that practically no deaths occur. Miss Shorten tried this at the experimental colony using rusk with ½% arsenic the first time and 10% about a week later. Of eleven rats identified after the first poisoning, one was completely shy of the plain base, the others, alas, took poison without any hesitation and all

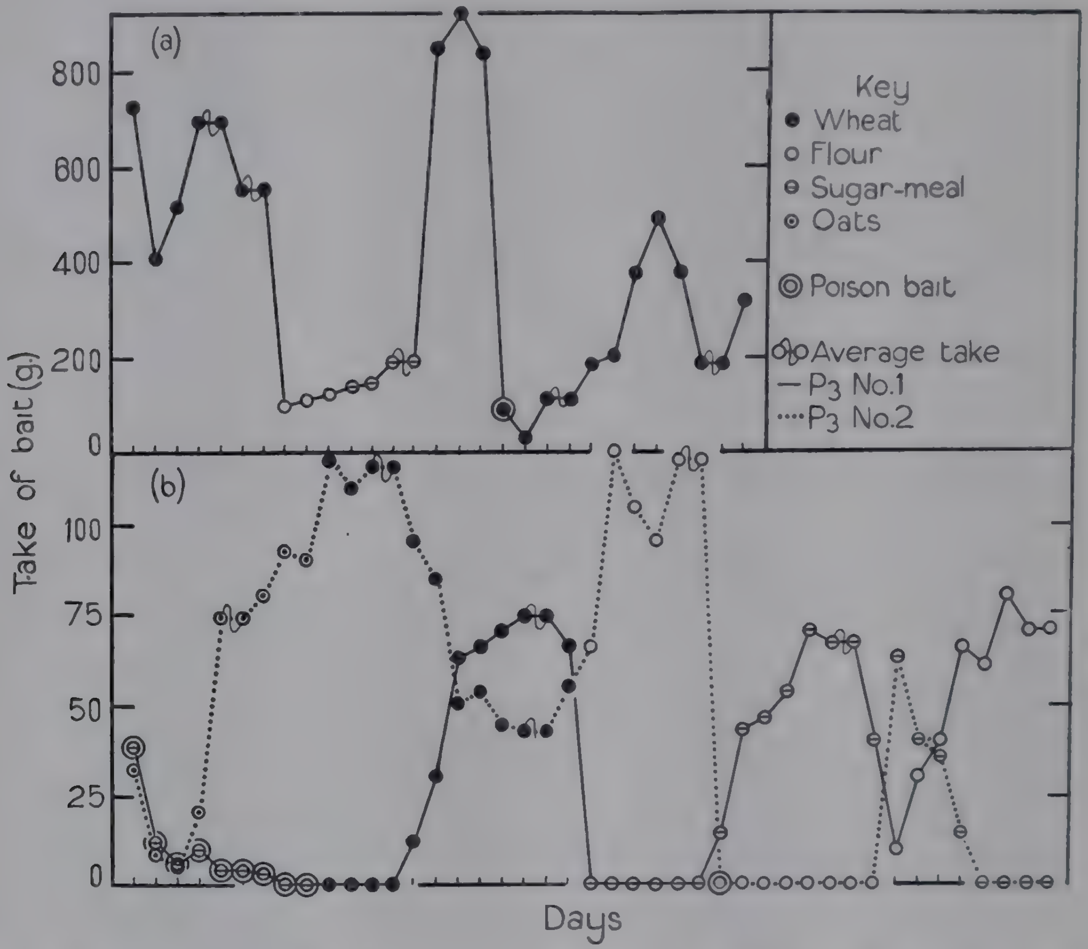

FIG. 17. Experiments by Ranson on shyness: daily takes of bait. (*a*) A wild population, survivors of zinc phosphide in flour 37 days before, largely refused plain flour or sugar-meal. A follow-up treatment with wheat and zinc phosphide was a failure. (*b*) Brown rats in an outdoor cage. Four out of seven survived 1% arsenic in sugar-meal given in one of a pair of P3s for 9 days. The curves show the sharp reactions of the rats to plain sugar-meal and their acceptance of flour until this was poisoned, when they returned to sugar-meal. Finally, the contents of the P3s were switched and the rats slowly returned to an exclusive diet of flour. Two rats disappeared and may have starved themselves to death rather than eat sugar-meal or flour after the latter had also been poisoned (there was no measurable take on this occasion.)

died; but the failure of the original bait to protect these rats may have been because it was removed as soon as they had finished their first meal. In the outdoor cages a low concentration of poison produced shyness with the loss of 3/7 rats (Fig. 17); but it cannot be assumed that subsequent behaviour will be the same as that following a bait of normal strength.

If a new object reaction can be produced on the night of poisoning the speed of feeding can be so reduced that illness sets in before a lethal dose is taken. On one occasion an entire population was thus made shy; at the experimental colony, by contrast, so little bait was taken in 48 hours from a newly placed tray that most of the rats were killed in an immediate follow-up with the same poison bait on the floor.

We have found that the least uncertain way of obtaining a residual population is by modifying direct poisoning so that bait is distributed in small amounts or at a restricted number of points or under other circumstances likely to prevent rapid feeding. Since some rats will not come across any bait at all the entire area must next be poisoned (without prebaiting); but even this procedure does not always prevent a heavy loss of animals for the crucial phase of the study (Ch. 14 and § 12 (*b*)).

Thompson has applied this idea in the outdoor cages with greater, though still incomplete success. Rats were given an abundant, unmeasured supply of wheat, cabbage, and water throughout. The first poison baits were placed without prebaiting and left for a few days at a distance from the nest box and food; nevertheless several deaths resulted from zinc phosphide, though not from arsenic. In later experiments an attempt was made to protect the animals by putting the bait in an unfamiliar tray. However, as shown by footprints, this arrangement could keep the rats away for as much as 10 days; whereas a simpler object did not slow down the feeding rate quite enough. To be sure that all the survivors had been reached by the first poison bait it was next given after prebaiting, and as already shown in Table 50 most of the survivors had become bait-shy.

These are the techniques which seem most promising, even though they did not always work, and we must now re-examine the problems to which they must some day be applied more thoroughly. The white rats studied by Rzóska (Ch. 8, Appendix 1) generally followed predictable lines of behaviour towards second poison baits. Rzóska found that the base rather than the poison was the part of the bait with which a rat associated its illness and that results of second treatments were as follows:

$$\left.\begin{array}{llll} \text{Same} & \text{poison} & + & \text{same base} \\ \text{New} & ,, & + & ,, \quad ,, \end{array}\right\}\text{refused}$$

$$\left.\begin{array}{llll} \text{Same} & ,, & + & \text{new} \quad ,, \\ \text{New} & ,, & + & ,, \quad ,, \end{array}\right\}\text{accepted}$$

Now the poisons on which these conclusions were mainly based were relatively pure and tasteless samples of arsenious oxide and barium carbonate and the bases had little in common; but in practice things may be different. Poisons such as zinc phosphide, for example, may have a strong enough taste or odour to be detected in a new base (Table 50); or two bases or two poisons may resemble each other in some way which a rat associates with its illness. Flour and sugar-meal are evidently too much alike for wild rats which are shy of one to accept the other; yet whole wheat is sufficiently different from kibbled wheat or wheatmeal to be accepted when they are not (Table 47). From Middleton's experience in the field and Rzóska's in the laboratory it seems possible that if sugar or aniseed are common to two baits the second one may be refused if the first has been poisoned. For all that is known to the contrary, common features such as texture, impurities, or chemical reactions between poison and starch may also be the means by which rats can sometimes avoid taking a second bait.

An important observation which needs to be confirmed is the failure of arsenic in sugar-meal after rusk but not vice versa. This example alone suggests caution in drawing general conclusions, and for the present the only safe course is to use an entirely different formula in second treatments.

With efficient operators and a choice of poisons and bases it should not be necessary to employ the same base and poison twice on the same population. However, in certain situations and in some parts of the world there is only a limited choice of bases and poisons. If only one of each were available, research would have to be directed towards finding out how long shyness may persist—and hence the most efficient interval between poisonings—and what concentration of poison produced the least shyness and yet gave reasonable results. If no shyness persisted after an initial treatment with a low concentration, the same base could be used again with the poison at normal strength. In this way better use might be made of limited supplies than by staking everything on one treatment which could not be repeated. The success of Doty's work may partly depend on the use of low concentrations of thallium sulphate and zinc phosphide.

Low concentrations are not likely to be essential for use in Great Britain, and as some rats may become base-shy it is necessary to accept the fact that certain bases at least must be changed. It would be of greater use if the same poison could be used again in different bases, and when the proper sequence is understood this may well be possible. This knowledge could be most usefully applied in the case of red squill since this is the only safe poison to use in some places.

Benjamin Broad, writing in 1813, has stated the problem of dealing with residual populations as follows: 'when their number is for a long time slowly diminishing the survivors grow suspicious, and it will sometimes require, in taking a hundred rats, more trouble to destroy the last five, than the preceding ninety-five'. In his case the animals were trap-shy, but many rat poisoners feel the same sense of frustration when trying to make a final clearance. We are still very much in the dark about the biological mechanisms responsible for bait-shyness; but we know enough to get over some of the practical difficulties (Ch. 10) and to suggest how further knowledge may be gained.

18. SUMMARY OF PART 2

1. The efficiency of various poisons after prebaiting was measured in a series of over 100 field trials. Arsenious oxide, zinc phosphide, red squill extract, and alpha-naphthylthiourea generally gave kills of 85–100%; red squill powder was less reliable. Barium carbonate in bread mash is of value under certain conditions only.

2. Eighty-three field trials of direct poisoning showed that kills were very variable, and that though good results were possible there was no way in which success could be guaranteed with the baits available for practical work in Great Britain.

3. The first important factor which limits the efficiency of poisoning is that illness may begin before a lethal dose is taken. Prebaiting increases the initial speed of feeding and ensures that a majority of rats eats a lethal dose. Highly toxic poisons and attractive bases should theoretically achieve the same result; but in practice they cannot always be used and much empirical work is needed to develop them.

4. Good results are obtained where prebaiting is done on 2 days with a gap between them and again before poisoning (1–3–5); some other systems may be equally good but cannot yet be recommended. There is a great advantage in intercepting the rats before they leave their burrows ('hole baiting').

5. The feeding behaviour of wild rats varies according to the individual and environment, and certain types of infestation are very troublesome. However, in the majority of British environments a good operator should have little difficulty in maintaining excellent control of the brown rat.

6. The second main difficulty about using poison is that survivors remain bait-shy and the most frequent cause of failure to maintain rat control is the incorrect use of poison in subsequent treatments. Some of the reasons why survivors are able to avoid later poison baits are known from laboratory work, but the field study of this problem is one of great complexity. For safety both base and poison should be changed in successive treatments; but as shyness varies with the base, the type and the concentration of poison and probably with its length of exposure, there are some conditions under which the poison at least may be used a second time.

PART 3

19. REVIEW OF SELECTED ASPECTS

From the start of our work we found that widely divergent claims, unsupported by evidence, were being made by various people practising rat control and that the literature seldom offered valid proof to support the recommendations. The first essential was therefore to apply objective methods of assessing field results; but it was found that the recognized methods of studying small mammal populations were mostly impracticable in the large number of routine tests that had to be made in all sorts of conditions. Rather similar conclusions were reached by Emlen, Stokes, and Davis (1949), McDougall (1946), and Lomakina (1936). We ourselves first adopted a prebaiting method resembling that used by Doty (1938, 1945) but later modified it to overcome certain errors. This method worked well under most circumstances in Great Britain and the Middle East. McDougall (1946), however, had less happy experiences: 'Sometimes unexpectedly large nightly intakes were recorded due to interference with the feeding stations by large fauna such as wallabies, rat kangaroos, pigs and scrub turkeys . . .', and later, 'attending feeding stations amongst high rat population densities under wet conditions was the filthiest work encountered . . .'.

Though our own troubles were less dramatic we also found census baiting unsuitable in some habitats and for assessing the results of large-scale control (Barnett, 1948 *a*). Nevertheless, census baiting has great advantages, being suited to a wide range of conditions, requiring little specialized training and reducing the subjective element to a minimum. The first part of this chapter has therefore been devoted to examining the validity of this system; for it is no contribution to substitute a doubtful set of quantitative data for verbal claims, which at least have the merit of being transparent. The stage to which we have taken the technique leaves room for much improvement, particularly in the accuracy of comparisons between different means of control. It has been fairly easy to prove that certain baits or procedures are unreliable, but not to distinguish between several which give 85–100% reductions. Since this is the range within which prebaiting methods now work it is clear that further progress partly depends upon better methods of testing.

One valuable improvement would be to contrast the results in two similar environments subjected to different treatments, for with sufficient replicates statistical tests could be applied. The chief difficulty in Great Britain is to find pairs of similar habitats and some attention might be given to a system of paired tests within populations. Neither of these methods has received more than preliminary attention here and the second method could not be undertaken without further study of movements of the type described in § 3 (*b*) and by Davis, Emlen, and Stokes (1948). Attention has been given to other problems of experimental design and though some of the examples have pitifully few replicates we were not free to seek intellectual satisfaction at the expense of practical objectives.

We must now review prebaiting as a method of control. Prebaiting is a means to an end, namely, to make rats take a large enough dose of a quick acting poison when they first meet it. However, this requirement is essential only if it is also true that the survivors cannot be killed later on by the same bait. So far, except by prebaiting, there is no known way of making certain that the first encounter is reasonably lethal or of killing the survivors except by a complete change of bait. It would be a great mistake, however, to think of these procedures as the final solution. For the present we must accept the fact that no single bait will make all rats eat it quickly enough and that high toxicity poisons are too dangerous and not fully effective. The foreseeable substitutes for prebaiting, then, seem to be limited to (1) making the poison act so slowly that the rats feel no effects for several hours or days, or (2) ensuring that the rats neither detect the poison nor associate their illness with the base. This second condition is not always understood, for it is incorrect to assume that because a rat is unable to tell the difference between plain and poisoned bait that the latter will remain acceptable. On the contrary, our own experience suggests that caged rats may starve themselves rather than eat certain plain bases with which they have ingested a poison (e.g. Fig. 17 (*b*); Ch. 8, Appendix 1).

We must now see what evidence there is that the new rodenticide Warfarin may fulfil the two requirements above. Warfarin, or Compound 42, is a blood anticoagulant, which produces lethal haemorrhages as a result of minute doses taken over several days. Here then we have a slow-acting substance which, in concentrations as low as 0·005 to 0·025%, may well be indetectable.

There is no need to dispute the first link in the chain of evidence, i.e. that Warfarin kills captive rats (Hayes and Gaines, 1950; Doty, 1951), though this is characteristic of most of the poisons already in use (§ 3 (*c*)). Captive rats, however, apparently continue eating Warfarin until they die, which is not so with other poisons. Hayes and Gaines showed that caged *R. norvegicus* given a choice of plain and poisoned food, did not switch their preference in the course of several days; and they conclude, no doubt rightly, that the poison was not detected. Since these rats had no alternative food this observation does not dispose of the hypothesis that the rats were shy of both plain and poisoned food and that their death was hastened by starvation. In Doty's tests caged rats (brown, black, and Alexandrine) were given sugar-cane as an alternative to rolled oats. Warfarin was then added to the oats and for the first day or so this bait was taken at the same rate as before. Doty states that Warfarin is thus the first poison not to be detected in some degree. He does not, however, provide the evidence that would clinch this statement (§ 15 (*c*)), namely, that there was no subsequent increase in the relative consumption of sugar-cane, or that this was an adequate food on which to keep rats alive in captivity. While agreeing, therefore, that these two papers offer exciting new evidence, we must base our hopes on the performance of Warfarin in the field. Here, if its properties are as claimed, really spectacular results may be expected.

Hayes and Gaines treated thirty-one infestations, picked up numerous corpses, eliminated damage to goods and, except in four sites, found that 'rat infestation was, in general, controlled within 2 to 3 weeks'. Some rat-free places became reinfested after a month or so. There is no need to doubt that the populations were severely reduced, but the evidence is inadequate for a quantitative comparison with other methods.

Doty has so far reported on three field tests only, on the island of Kauai. In one case rolled oats was taken in increasing amounts up to and including the first day or so after Warfarin was added. The second test merely confirmed that after the ninth day there was little further take; neither test gave any evidence about the size of the residual populations. (Presumably it was inferred that if caged rats could not distinguish between plain and poisoned oats any survivors in the wild would have continued taking the poison bait.) In the third test a postbait was carried out and takes slowly rose to 1,790 g. oats per day. Doty does not discuss the possibility that the 200 to 250 rats represented by this take might have included survivors; all are assumed to have been immigrants. Therefore this example proves only that critical tests should not be carried out where there is likely to be re-invasion.

Another test, on the island of Hawaii, was done by Gross, Baker, and Bonnet (1951) who trapped at the baiting sites for a week before poisoning and for 4 weeks afterwards. Two criticisms may be made of this method, namely, that the traps were not distributed at random within the experimental area (no doubt a difficult job) and that no control area was set aside. There was no apparent success against *R. hawaiiensis* since more were trapped after the poisoning than before. The following catches of other species were made in the week before and in the week after: *R. alexandrinus* (76:4); *R. rattus* (8:0); *R. norvegicus* (21:2). As an index of success the validity of these figures rests on two assumptions: that no trap- or place-shyness had developed and that the trapping itself had not been responsible for gross changes in numbers. On the other hand, the apparent failure against the native rat is also open to question because of errors similar to those illustrated in Fig. 3 (*b*).

The three papers so far discussed show how faults in experimental design can raise doubts, at least in the mind of an outsider, about conclusions from important work. These studies should be contrasted with two others on blood anti-coagulants. Schein (1950) in Baltimore used the census methods reviewed in § 3 (*c*) and made parallel observations on control areas, an excellent feature of this and some other Baltimore tests. An average kill of just under 85% was achieved on brown rats. Armour and Barnett (1950), in this country, used census baiting and showed that the claims of O'Connor (1948) in favour of dicoumarol could not be substantiated. The results were poor with wild brown rats and they became bait-shy.

Further quantitative evidence is needed about Warfarin as it is, in theory, an excellent substitute for prebaiting, or at least for the tedious maintenance treatments. However, until more data are available, prebaiting with recognized poisons must be recommended although we know that this system does not work everywhere (§ 15), that it will not succeed in unskilled hands (§ 16), and that it is quite unnecessary if the right bait is used under the right conditions. So far we do not know the secret of this last combination.

This and many other fascinating problems remain, and the brown rat is a particularly good species to study because of the amount of knowledge about the domestic strain. Some of the physiological and psychological conditions produced experimentally have already been shown to have their counterpart in the wild. A great advance in knowledge of the wild species is to be expected from closer co-operation between the field worker and experimental zoologist and from a more cautious testing of laboratory conclusions before they are applied.

The study of the brown rat is, of course, chiefly important for economic reasons, and though this aspect has not been stressed in the present chapter an idea of the size and cost of rat control campaigns will have been gained from the figures quoted in § 3 (*b*) from Long (1931) and in § 12 (*d*) from Dopmeyer (1936). Examples of other large-scale campaigns are given by Cilento (1936), Eskey (1934), and Barnett (1947, 1948 *b*). In comparison with the magnitude of the problem there have been surprisingly few facts on which to plan the most efficient attack.

APPENDIX 1

FIELD TESTS OF POISONING

TABLES 51–54 include all tests which fulfil the requirements discussed in § 11 (*a*). Most tests were done in rural environments; similar results were obtained in towns but it is less easy to present valid evidence (§§ 4 (*b*), 16 (*d*) (i)). The Oxford market was the only place where there was any obvious correlation between results and habitat type. The 163 tests which are summarized in Table 25 were carried out in the following habitats: 'farms' (70); ricks of corn or legumes (26); hedges, ditches, &c. (23); threshed rick sites, baled straw, &c. (13); rubbish dumps (8); food premises (8); miscellaneous (15), including railway yards, a hotel, disused kennels, a sewage farm, knackers' yards, &c. (From urban poultry pens upwards, all places concerned with domestic stock are classified as 'farms'.)

The following abbreviations have been used in Tables 46–55:

Bases

B = bread mash: one part bread with an equal weight of water (Ch. 7).
F = flour, plain wheatmeal (Ch. 7).
K = kibbled (broken) wheat.
R = sausage-rusk with an equal weight of water, or with twice its weight where stated 2:1 (Chs. 7, 10).
S = sugar-meal: 1 part by weight of castor sugar to 9 of flour (Chs. 7, 10).
Sc = ,, with 0·5% chlorazol blue 3BS (Chs. 2; 4, § 3 (*c*)).
SB = soaked barley
SM = ,, maize (Chs. 7, 14).
SW = ,, wheat
W = whole English wheat.

Poisons (Ch. 2)

α = α-naphthylthiourea.
a = arsenic (arsenious oxide).
b = barium carbonate.
e = extract of red squill (alcoholic).
p = powdered red squill.
w = water extract of red squill.
z = zinc phosphide.

The samples of poison are discussed in Ch. 2 or Ch. 4, § 11. Arsenic samples 1, 8, and 20 were fine particle sizes, 7 was coarse, 5, 24, and 31 were similar medium arsenics, 26 was a very pure grade of it. Red squill powders S 1 and 5 had L.D. 50 of <500 mg. per kilo; the L.D. 50 of S 2 M.I. was >1,250 and that of S 6 was 857 mg. per kilo. Barium carbonate was either precipitated (ppt) or the natural witherite.

Other terms are '*x*%' or '*x*% conc.', showing the per cent. by weight of poison in the total weight of bait; peak = peak take of wheat in census 1; % kill = reduction of the population as estimated by census baiting (§ 5) or by dusk counts (§ 3 (*b*)) or known from a count of the total population (§ 3 (*a*)).

TABLE 51. *Field tests of poisons after prebaiting, mostly on the 1–3–5 system. Measured by census baiting*

(a) *Arsenic*									(b) *Zinc phosphide*			(c) *Red squill powder*			(d) *Red squill extract*		(e) *Antu*	
% *conc.*	*Base*	*Peak*	% *kill*	*Sample*	*Base*	*Peak*	% *kill*	*Sample*	*Base*	*Peak*	% *kill*	*Peak*	% *kill*	*Sample*	*Peak*	% *kill*	*Peak*	% *kill*
						(*10% conc. only*)				(*5% conc.*)		(*10% conc. in rush*)			(*5% in rush*)		(*1 or 2% in rush*)	
10	S	1,290	96	1	SW	245	100	31	S	400	55	485	94	S 1	1,620	67	1,520	78
10	S	380	91	8	SW	1,360	100	31	S	1,310	100	840	96	S 1	765	*c.* 50	420	100
15	S	610	94	20	SW	1,150	96	31	S	720	100‡	570	89	S 1	295	*c.* 50	(*2% in s.-meal*)	
15	SW	630	94	20	SW	360	100	31	S	600	100‡	2,270	83	S 1	(*10% in rush*)		115	100
..	..	..	..	..	SW	485	100	31	S	290	100‡	470	100	S 1	760	86	165	100
15	S	3,025	84	5	SW	450	100	31	SW	220	100	*c.* 1,050	< 85	S 1	650	96	1,615	94
15	S	295	100	31	SW	1,035	88	31	R	200	65	660	83	S 1	1,090	98	140	100
15	S	260	100	26	SW	1,095	< 55*	31	R	5,480+	> 97	2,100	79	S 5	225	58	525	< 70*
15	S	445	90	26	SB	1,860	95	31		(*2% conc.*)		360	86	S 5	150	80		
15	S	2,870	100	26	SB	500	95	31	R	1,225	95	380	72 †	S 5	170	100		
15	S	270	100	26	SB	2,740	87	31	R	500	97	1,920	78	S 5	175	100		
15	S	410	100	26	SB	350	100	31	R	550	100	840	55	S 5	3,710+	> 87		
15	Sc	1,070	100	26	R	1,500+	< 70*	31	R	140	100	340	62–53	S 2 M.I.	860	93		
15	Sc	170	88	26	R	780	88	5	R	640	91	450	93–68	S 2 M.I.	2,020	85		
15	Sc	110	100	26	R	1,280+	100	24	R	285	86	1,070	70–42	S 2 M.I.	2,150	96		
15	S	725	91	26	R	310	100	31				275	59–31	S 2 M.I.				
10	SW	570	91	31	B	640	100*	31				940	93	S 6				
10	SW	370	60	31	..	..	..	..				990	95	S 6				
10	SW	1,080	100	31	S	785	100	7				4,390	60	S 6				
10	SW	110	100	31	..	..	..	..				1,190	82	S 6				

* Oxford market. † With 10% sugar in prebait and poison bait. ‡ Three-week-old baits.

(The lower estimate of kill with S2 M.I. is based on > 5 days postbaiting.)

TABLE 52. *Field tests of poisons after prebaiting on the 1–3–5 system. Measured by dusk or total population counts*

(a) Arsenic				*(b) Zinc phosphide*			*(c) Red squill extract*		
Count	*Rats*	*% kill*	*Sample*	*Count*	*Rats*	*% kill*	*Count*	*Rats*	*% kill*
	(15% in s.-meal)				*(5% in s.-meal)*			*(10% in rusk)*	
Total	18	72	20	Dusk	59	98	Total	19	95
				,,	64	100	,,	16	100
Dusk	22	32	5	,,	37	95			
,,	14	100	5	,,	80	91		*(d) Antu*	
	(10% in s.-meal)				*(5% in flour)*				
,,	21	95	5	,,	14	86		*(2% in s.-meal)*	
,,	16	81	5	,,	32	81	Total	8	87
,,	22	73	5	,,	32	78			

TABLE 53. *Distribution of results with medium arsenic and zinc phosphide used in ricks on the 1–3–5 system (from Ch. 10, Table 1)*

% kill:	*70–84*	*85–100*	*Total mortality*
Arsenic in sugar-meal	0	3	99/111
Zinc phosphide in sugar-meal	2	9	> 301/> 329
Zinc phosphide in rusk	1	2	66/75

TABLE 54. *Field tests of direct poisoning with zinc phosphide or Antu (see also Tables 4 and 5)*

Census baited tests						*Dusk or total population counts*				
(a) Zinc phosphide				*(b) Antu*		*(c) Zinc phosphide*				
Peak	*% kill*	*Peak*	*% kill*	*Peak*	*% kill*	*Count*	*% conc.*	*Base*	*Rats*	*% kill*
(5% in s-meal)		*(5% in rusk)*		*(2% in s-meal)*						
c. 300	100	220	100	150	93	Total	10	S	229	91
110	100	160	100	1,250	77	,,	5	R	37	89
410	89	580	71	*(2% on s. wh).*		Dusk	2	R	28	89
530	85	2,460	50	325	100	Total	2	R	16	88
500	84	310	49	980	92	Dusk	10	S	18	83
340	78	9,225	48	*(2% in rusk)*		Total	2	R	5	80
220	73	400	26	3,430	92	,,	5	R	118	72
						,,	10	R	13	0

TABLE 55. *Field tests of bread mash with 10% medium arsenic or 20% barium carbonate: mostly after surplus prebaiting in containers*

(a) Arsenic			*(b) Barium carbonate*		
*Peak take of wheat (g.)**	*% kill*	*Sample*	*Peak take of wheat (g.)*	*% kill*	*Sample*
480	100	5	335	76	ppt
400	96	5	115	84†	,,
190	100	5	265	68	,,
333	100	5	140	100	,,
133	100	5	240	84†	,,
210	100	5	520	65	,,
200	100	5	270	62	,,
200	poor	5	340	77	with.
100	100	5	110	100	,,
450	100	24	180	100	,,
			1,785	55	,,
			325	100	ppt

* Figures in italics are either the expected takes derived from the take of bread mash prebait × $\frac{1}{3}$, or the minimum expected takes derived from the take of poison bait × $\frac{5}{3}$ (Ch. 7).

† 10% barium carbonate.

APPENDIX 2

ANALYSIS OF A MARKED POPULATION

(*a*) **Statistics**

BODY weights of five marked rats had increased between live trapping on 25/ January and recovery of the corpses on 16/17 or 22/ February (Table 32). The regression of final on original body weight gives $b = 0{\cdot}6215 \pm 0{\cdot}07128$ (t for 3 degrees of freedom $\doteqdot$ 5 and $P < 0{\cdot}02$). The unknown weights have been corrected accordingly in calculating the takes of poison per kilo body weight on 16/ February. For 9/10 February it has been assumed that two-thirds of this total body weight had been regained. This is an arbitrary figure based on the data for half-starved rats which had made 60% of their final gain in weight after 5 night's refeeding. Both corrections are highly approximate but give more conservative estimates of take per kilo body weight.

Table 31 shows the proportion of feeding visits made by marked rats on those prebaiting occasions when complete counts were made. Similar results were obtained on the night of poisoning (81 out of 140 visits, or 58%, were by marked rats). Despite the high degree of individual variation in behaviour the ratios are unexpectedly consistent and make it worth seeing what confidence can be placed in population estimates derived from them. Dr. D. J. Finney has kindly prepared the following comments.

Of the total of 2,578 visits recorded during the two nights, 1,416, or 54·9%, were by the eleven marked rats. A χ^2 test of the variation in this percentage during the fourteen different periods of recording showed significant heterogeneity. As might be expected, visits were not entirely at random times, but any one rat tended to make a series of visits in a short interval, so increasing the variation in the proportion of visits by marked rats. For one exceptional hour (6·00–7·00 a.m. on /10 Feb.), the percentage fell to 28·7%, and otherwise it ranged from 46·3% to 67·8%. The standard error of the mean, estimated from the weighted sum of squares of deviations about the mean (Snedecor, 1946, § 9. 10), is ±2·9%, so that limits within which the true value of the mean is almost certain to lie may be assessed as 61·3% and 48·5%.

On the assumption that individual marked and unmarked rats over a long period visit the bait with equal frequencies, this result may be used to estimate the total population. If the eleven marked rats form about 54·9% of the population, the total must be estimated as twenty, with limits of error similarly calculated as eighteen and twenty-three.

(*b*) **Individuals**

No. 0. 160 g. ♂ (Fig. 14). On 6/ and 9/ February and again on the poison night this rat appeared without eating from the tray at first. No doubt it picked up food particles dropped by other rats and this is a possible reason for its survival of the poison. The difference in feeding tempo between its hungry and well-fed state was very striking. It showed no hesitation in taking plain rusk but only returned once that night after its first 10 mins. It fed well the next night (but some of its visits were confused with those of another rat and have been omitted from Fig. 14). During the postbait it was far more consistent in its initial feeding than at any time since token baiting, no doubt because there was no disturbance from other rats. Although it was one of the smallest rats its mouthful of wheat postbait was about average weight. When trapped on 22 February it weighed 265 g., a 105 g. increase.

No. 1. 160 g. ♀. The advantages of baiting a hungry rat are very clearly shown with this one which fed for at least 20 minutes when hungry, but only intermittently throughout surplus prebaiting. On 7/ February it took one mouthful at 7.20 and disappeared for 3 hours. During poisoning it did not feed from the tray but was seen a total of nine times and evidently picked up a lethal dose from crumbs dropped by the other rats.

No. 2. 190 g. ♂. The behaviour of this rat upset all conventional notions about prebaiting. The first night it passed through the shed without stopping for a single mouthful or even a sniff. The second night it arrived at 10 p.m. and fed four times during three periods separated by 20-minute intervals. It was not seen during the first two days of surplus prebaiting, but fed quite well on the third night though arriving late. During the all-night session it fed nine times (*c.* 4 g. take). It did not appear the first time rusk was supplied, but fed remarkably well late the second night and took more mouthfuls of poison than any other rat. There is a suggestion here of indifference to wheat and attraction towards rusk.

No. 3. 290 g. ♂. Once this rat started on wheat it fed for at least half an hour continuously. The only exception was on the night after the first surplus when it was presumably surfeited. It occasionally remained for about ¼-minute settled feeding on its visits to collect mouthfuls, which it invariably consumed behind the same piece of cover. During the all-night session it stayed away between 8.30 p.m. and 3 a.m., the longest absence of any of the marked rats. It apparently made up for this by being the only intensive feeder between 7 and 8 a.m. Its feeding on rusk was more intermittent than on wheat, but on the second night its copulations with No. 4 may have distracted it. It consumed seven mouthfuls of poison bait and then, within 10–15 minutes pawed over the pile and refused any more. It had taken a dose of about 520 mg. per kilo original body weight or 430 g. per kilo corrected body weight.

No. 4. 390 g. ♀. This rat was the most settled feeder of all, but far more so during token than surplus baiting. On 5 Feb., for example, it fed almost entirely at the pile although running in and out quite often. On 6/ and 14/ February this female was often involved with a large unmarked male. Most of the time the rats sat and watched one another like two fighting cats, they advanced and retreated, touched noses and once fought. Much of this was done in the main entrance hole and interfered at times with the other rats' feeding. The male emitted frequent little guttural noises. Between 7 and 11 p.m. on 14 February No. 4 copulated at least four times with this large male, once with another unmarked animal, once with No. 6, three times with No. 3, and on other occasions was chased outside.

No. 5. 300 g. ♀. This animal always came from the north-east corner and on its first feeds on token bait made a greater number of total visits and at a higher rate than any other rat. It never sat down at the tray. It made twenty-one visits in 5 minutes during token baiting compared with a maximum of twelve during surplus baiting with wheat. It took exceptionally well to rusk, four times reaching seventeen visits per 5 minutes. When it first approached the zinc phosphide it smelt it and shot back. Although it quickly recovered from its surprise it took only six mouthfuls and stopped within 10 minutes. Its stomach was found to contain other foods and the amount of poison bait could not be determined, though presumably it was considerably less than 380 mg. per kilo as worked out from the number of mouthfuls. Its weight on 17 February was 340 g.

No. 6. 280 g. ♂. This rat was a very good feeder when token baited. It ate much less rapidly and continuously when surplus baited and largely abandoned the settled habit. It apparently took well to rusk at first but ate slowly and intermittently the next night, possible because of the counter-attraction of No. 4 who was in heat. It took the poison well.

No. 7. 260 g. ♀. This rat tended to feed slowly over a long period and, except during token baiting, with frequent short absences. On 9 February an absence of 3½ hours after the first mouthful was exceptional. It seemed to take well to rusk but its slow tendencies evidently operated on the poison night when it took a single mouthful. It was seen occasionally thereafter and may have been scavenging: in any case it was avoiding the poison bait after 1 hour 10 minutes and looking for old residues of plain rusk. It was seen the following night. Twice it cautiously approached the tray but withdrew without getting closer than 2 in. The second postbait night it had regained a little confidence but after five visits took nothing more in the following hour. Trapped on 22 February it weighed 360 g.

No. 8. 300 g. ♂. During token baiting this rat appeared to be much less nervous than it did afterwards, particularly on 6/ February when it approached and retreated many times and its feeding pattern was very intermittent. Like some other rats it also took a single mouthful on one occasion and then disappeared for several hours. With rusk it appeared late the first night but fed extremely well the second. On the poison night it started feeding 40 minutes later than any other rat, and though seen before then it evidently got no crumbs of poison.

No. 9. 240 g. ♂. Until 9/ February this rat's behaviour was most remarkable: it was seen to feed from the tray five times only. On 7/ February, for example, out of sixty times seen it fed only once at the tray. Instead it used to scavenge the grains dropped by other rats or sit down and share their mouthfuls; it regularly inspected behind the cover used by other rats. Then on 9/ February it changed this behaviour completely and fed like all the rest. On the first night of rusk it took one mouthful only at 7.30 and was seen no more (up to 11 p.m.). The second night it was seen once only. Yet on the night of poisoning it took the second highest number of mouthfuls. Its corpse was found 3 hours later and 10 g. poison bait was recovered. The rat's weight had increased to 300 g.

No. 10. 300 g. ♀. On all except one night with wheat its peak rate came within the first 15 minutes of its start. This changed when rusk was substituted: the first night it fed once only in the first 4 hours and the second night its feeding was spread over several periods with a peak rate during the third hour. It took only one mouthful of poison, but died. There is a strong suggestion here of a dislike or suspicion of an unfamiliar food.

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